Vol. XXX
No. 3

PSYCHOLOGICAL REVIEW PUBLICATIONS

Whole No. 136
1921

Psychological Monographs

EDITED BY

Critical and Experimental Studies in Psychology

From the University of Illinois

EDITED BY

MADISON BENTLEY

PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J.
AND LANCASTER, PA.

AGENTS: G. E. STECHERT & CO., LONDON (2 Star Yard, Carey St., W. C.)
PARIS (16, rue de Condé)

TABLE OF CONTENTS

DYNAMICAL PRINCIPLES IN RECENT PSYCHOLOGY

By Madison Bentley

and

Members of the Psychological Seminary

I.

Man's earliest conception of mind appears to have been that of an intangible and inscrutable substance which was at once an active personal agent, a private possession, and a unique source of power and authority. The more detached and less emotional accounts of mind, accounts which have put description before possession and understanding before use, arose, as a natural sequence, very much later. But the two views once established became enduring and persistent rivals. To this day they have contended together.[1] The earlier view has been generally represented in the "doctrine of the soul,"—a doctrine encouraged by philosophy, theology, and the unreflective opinion of common sense and uncritical knowledge. The strongest support of the later view has come from the natural sciences, which have lent it both a pattern and a methodological background. The development of the physical sciences has, it is true, laid stress upon a description of the world in terms of the transfer of energy within and between systems,—a description which has seemed to leave mind out of account. At the same time, the insistence of these sciences upon controlled and verifiable observation of events has been carried over to the mental facts and thus has indirectly promoted a descriptive psychology of process.

[1] Writers who look upon the dynamical conceptions as new and modern display a distorted perspective. H. W. Carr, *e.g.,* declares in the Preface to his English translation of Bergson's *L'energie spirituelle;* "In recent years we have witnessed the opening up of a new and long-unsuspected realm of fact to scientific investigation, the unconscious mind. The very term seemed to the older philosophy to imply a latent contradiction, today it is a simple general description of recognized phenomena."

The older, active or dynamic view of the "soul" has not, in spite of the scientific trend of our own generation, been given up. All sorts of extra-psychological sanctions—philosophical, biological, medical, etc.,—have sustained it. With these latter sanctions we are not here concerned. But psychology itself has found, and it continues to find, reasons for conceiving mind as a power or force, as an agent which originates, directs and controls. These reasons are many and varied; nothing less than a history of modern psychology could expound and interpret them. Our own study is much less ambitious. It proposes to examine a few outstanding accounts of mind which have been substantially based upon dynamical principles. These accounts represent many schools and various traditions, and they sustain unlike interests in the facts and laws of mind. By "dynamical" we mean that they consider mind under the category of *activity:* that they represent mind as being or possessing a central force or power which is causally related to other forms of existence and to physical events. Our examination of these chosen systems has made it appear that the dynamical elements in recent psychology are of at least four kinds; *i.e.,* mind is regarded as a creator, an initiator, a selector and repressor, and an organizer. Possibly these forms of activity differ only in degree and in shading, and some writers pass easily in their expositions from one of them to another; but they seem to represent logical differences as well as differences in point of view and perspective which have not been carefully defined.

We have chosen men who have either exemplified important historical doctrines or who have represented an aspect or phase of psychology of serious import to the developing and expanding science. Their own distinct contributions to psychology have been by no means equivalent. The writers chosen for critical study in the seminary are James, Woodworth, Janet, Bergson, Freud and McDougall.

II.

William James

(C. A. Ruckmick)

There seems to be no ambiguity concerning the general type of mind that James describes in his *Principles of Psychology.* It is always an active, energetic, dynamic agent of the psychophysical organism. Statements illustrative of his point of view, taken from various contexts, are:

"Consciousness is *in its very nature impulsive*" (II, 526). "The *impulsive quality* of mental states is an attribute behind which we cannot go" (II, 551). "This dynamic (I had almost written dynamitic) way of representing knowledge has the merit of not being tame" (I, 369). "To my brain, however, I am dynamically present, inasmuch as my thoughts and feelings seem to react upon the processes thereof" (I, 214).

When we proceed, however, we find here and there points that are not so clearly made and distinctions that are not so sharply drawn. But the primary descriptions, even in detail, are emphatic.

First of all consciousness in its several phases is a *selecting* agent.

"Consciousness is at all times primarily *a selecting agency.* Whether we take it in the lowest sphere of sense, or in the highest of intellection, we find it always doing one thing, choosing one out of several of the materials so presented to its notice, emphasizing and accentuating that and suppressing as far as possible all the rest" (I, 139).

Selection goes on specifically on all planes of mental activity.

"To begin at the bottom, what are our very senses themselves but organs of selection" (I, 284). "The mind selects again. It chooses certain of the sensations to represent the thing most *truly,* and considers the rest as its appearances, modified by the conditions of the moment" (I, 285; cf. I, 78). "Out of all present sensations we notice mainly such as are significant of absent ones; and out of the absent associates which these suggest, we again pick out a few to stand for the objective reality *par excellence*" (I, 286). "It [thought] is interested in some parts of these objects to the exclusion of others, and welcomes

or rejects—*chooses* from among them, in a word—all the while" (I, 225; cf. I, 284).

Again aesthetic unity is said to be due to the mental elimination of discordant effects and "ascending still higher, we reach the plane of Ethics where choice reigns notoriously supreme. An act has no ethical quality whatever unless it be chosen out of several all equally possible" (I, 287).

All of this is nothing more than an elaboration of James's original thesis, that mind can be detected by two tendencies, (1) the pursuance of future ends, and (2) the choice of means for their attainment (I, 8, 11). Consciousness is efficient in these offices throughout: it is a fighter for ends and is endowed with causal efficacy in the fight (I, 141-143). There seems to be no doubt in James's opinion of the mind's purposive character as a background for the operations of choosing just described or of the mind's power to enact what it chooses to select. The point at which consciousness makes itself directly felt as a power is in volition and attention. Here there seems to be some systematic ambiguity, however, for we learn at one place that attention is not a new or mechanical force, but an effect produced by the environment (I, 450-454), and at another that it is in part a force, spiritual in its essence (I, 454, 468). And so with volition; for we read that "will is nothing but attention" (I, 447), and that volition involves an effort to attend and a consent (II, 568).

As a matter of stern fact James admits that psychology, the empirical science, must yield the question of conscious initiation of process through the will to the realm of speculation. It is more satisfactory to hypothecate, says James, a consciousness that can step in, through the will to attend, and regulate the flow of the mental life, even if it "could only be to hold some one ideal object, or part of an object, a little longer or a little more intensely before the mind" (II, 576). By a faith grounded in other than empirical or even logical considerations, then, consciousness is also an initiative agent. It is capable of adding impetus out of its storehouse of reserve energy whenever the odds are against it. In detail, "nerve-currents . . . . must in this

case be supposed strengthened by the fact of their awaking one consciousness and dampened by awaking another. *How* such reaction of the consciousness upon the currents may occur must remain at present unsolved" (I, 142).

Consciousness is, then, primarily an actively directing agent. It continually selects both among its own processes and indirectly, through its supposed influence on the nervous system, among physiological processes. This form of initiation, however, is more regulative in action than it is wholly creative.

R. S. Woodworth

(Mary A. Henry)

Professor Woodworth has recently written an outline of "Dynamic Psychology"[2] The book is, as the author says, a study in the "workings of the mind" (43). It regards in an active and energetic way the causal relations of mind and behavior. Those bodily instruments and organs which are involved in the neural and motor functions it conceives as "mechanisms" while the *causes* of neural discharge and the motives and springs of action, on the other hand, it calls "drives." The drive in a machine, the author describes as "the power applied to make the mechanism go" (37). Stimulus is drive; one part of the nervous system may drive another; the "inner tendency" toward reaching a goal is a drive; mental processes possess an "inner dynamics" (38-43); willing is the development of fresh motive power (149); reasoning implies an "access of energy" in an "obstructed tendency" (147), and the "higher and more inclusive self" is capable of resolving inner conflict and so of making the individual free (152). So many forms has the "drive." At times it is obviously physiological, representing the release or the initiation of energy in some part of the nervous system; but in other contexts it appears to bear a direct reference to mind,—as in the motive to selective action, curiosity in learning, interest sustained in objects and pursuits, and impulses directed toward the conquest of obstacles.

This dynamical factor, represented in Woodworth's "drives,"

[2] Woodworth, R. S., *Dynamic Psychology,* New York, 1918.

is not easy to define. Often it appears to be little more than a faculty to which, as a cause, observed facts are referred;—so the "motive forces" of the native equipment of men and animals, the "innate tendencies," "instincts," and "special capacities." Observed facts are referred to unknown forces, tendencies and potentialities, which then become hypostatised as drives, not unlike the traditional faculties and the mental powers and capacities of Gall and Spurzheim. Because of their conceptual and hypothetical origin, it is difficult to say whether, or just when, they belong to "consciousness" and when to bodily function. So far as mind is implied, Woodworth's use of dynamic principles appears to suggest *initiation* and *selection,* very much as we have found these things in the writings of William James.[3]

Pierre Janet

(Elizabeth Rutherford)

A characteristic type of mental activity, a type which has appeared in various psychological—and especially psychopathological—contexts in the last generation, is represented in the writings of Dr. Pierre Janet, a pupil of Charcot's. It appears in Janet's works as early as the 80's, where the French physician seeks a psychological basis for the symptoms of hysteria and allied disorders; *i.e.,* for the "automatic" phenomena of catalepsy, somnambulism, anaesthesia, and the like.[4] Janet's temper, as well as his training and traditions, has disposed him to reject the physiological or "reflex" explanation of these disorders, as proposed by Haidenhain, Maudsley, and Despine (*L'autom.,* p. 21). He prefers a "psychological" explanation. It is, as he

[3] The influence of James is everywhere apparent in Woodworth's little book. It may also be that the author's association with Ladd in the "Elements of Physiological Psychology,"—where the causal efficacy of mind had received a liberal interpretation,—inclined Woodworth toward this form of dynamic doctrine. Again, it appears that this and many other "genetic" accounts of mind, especially those prepared for educational purposes, easily turn to account the biologist's dynamic faculties of innate and inherited capacities, instincts, and powers.

[4] *L'automatisme psychologique; essai de psychologie expérimentale sur les formes inférieures de l'activité humaine,* Paris, 1889.

thinks, just in these abnormal states that the simplest and most rudimentary facts of mind come to light; and for the understanding of them, he appeals to a distinction many times recognized in the history of philosophy, and specifically formulated by Maine de Miran;[5] the distinction, namely, between bare sensation ("la sensation sans conscience, san moi capable de l'apercevoir") and the self or person ("une personne, un moi constitué un, simple, identique . . . . la conscience complète").

Now it is this "conscience complète," the "personal consciousness," and its defects in disease and disorder which form the basis of Janet's psychology of the abnormal.[6] That is the active thing which unifies and organizes experience. When the organizer is absent or disturbed, the morbid features of hysteria, hypnosis, catalepsy, and the like, appear. In such grave states as catalepsy, the personality is wholly wanting; mind is reduced to a state "purement affective, aux sensations et aux images." From this total "automatism" of catalepsy, Janet proceeds to build up the organized mind through the incomplete syntheses displayed in somnambulistic and suggestible states, and in the partial automatism of subconscious acts and local anaesthesias. As the exposition proceeds, the antithesis grows between automatism, the sheer existence of sensations and images, and the synthetizing operation of personality. The latter is not a mere principle of association; it is "an activity which synthetizes at each instant of life the various psychological phenomena and which forms our personalized apprehension (perception personnelle) of things" (*L'autom.*, 307). More and more the notion of a power or faculty, as opposed to the passivity of "mere sensation," developes in the exposition, and more and more it appears that the mental disorders are not primarily disorders of process or of

[5] In *Anthropologie* (*Œuvres inédites,* 1859, iii), 362, 405.

[6] Janet acknowledges (*L'autom.*, 399) an anticipation of his general doctrine of ruptured personality in an anonymous brochure of 1855: *Seconde lettre de gros Jean à son évêque au sujet des tables parlantes, des possessions et autres diableries.* Spiritistic exhibitions are here explained by the assumption that the organism is directed "par l'intelligence sans l'intervention de la volonté. In his acknowledgment Janet suggests,—perhaps more pointedly than he realizes,—the derivation of his dynamic principle from the Wolffian faculties.

the automatic ligation or "association" of processes, but rather defects or "lesions" of a power (puissance), "la faculté de synthétiser les sensations en perception personnelle (*ibid.*, 314), which lead to a real disorganization or "dis-assembling" (désagrégation) of mind.

In his later works[7] Janet applies in various directions this central notion of a synthetizing activity which is weakened, disturbed, divided or broken down in hysteria, "double personality," psychaesthenic states, neurotic conditions, and so on. There is apparent a tendency, as time goes on, to increase the emphasis upon neurological descriptions,—as in the inception of hysteria, which is defined as a "depression, an exhaustion of the higher functions of the encephalon" (*Major S.*, 333) and in the use of such vitalistic terms as "nervous strength," "nervous tension," anatomical "system" and "associations' (*ibid.* 180). Moreover, the frequent use of "dissociation," "mental depression," "tension" and changing "mental levels" has a less dynamic sound than the older expositions. Nevertheless, the main conception of "personal synthesis' remains. It is a dynamic concept which is now very widely used in the pathological literature, where it stands closely related to the notions of "dissociation" and of the "subconscious." It is in its essence a faculty of organization; though it inclines here and there (as when related to the will) to assume the rôle of a creator.

Henry Bergson

(Coleman R. Griffith)

When we turn to Bergson we pass, of course, beyond empirical psychology to a general, voluntaristic account of the universe; but Bergson's philosophy is so intimately bound up with historical trends in psychology and in the science of life that his exposition of "creative evolution" falls naturally under our present process of sampling. We may leave aside the philosophical

[7] *The mental state of hystericals, etc.,* (Corson, C. R., trans.), New York, 1901, pp. 492, 502, 527; *Névroses et idées fixes* (2 vols.), Paris, 1904, 1908; *Les obsessions et la psychasthénie* (2 vols.), Paris, 1903; *The major symptoms of hysteria,* New York, 1907, 311, 332; *Subconscious phenomena,* Boston, 1910, 53-70.

antecedents and implications of Bergson's theories; but we shall have to consider the relations of his system, first, to that psychology of the abnormal which has for many years in France capitalized the unconscious and the automatic and, secondly, to the current vitalistic doctrines of the biologists.

In Bergson's conception of mind we seem to see Janet's fundamental antithesis of "personal consciousness" and "automatism" spread out upon the whole wide canvas of the universe. At the centre of things is life, the creative impulse, which integrates, organizes, constructs, constantly creates. Life is also mind: it is will: it is struggle: it is opposed to matter. "Consciousness has had a narrow escape from being itself ensnared. Matter, enfolding it, bends it to its own automatism, lulls it to sleep in its own unconsciousness." The history of life is the history of the struggle of consciousness to free itself from "automatism and unconsciousness." "Freedom is riveted in a chain. . . . With man alone a sudden bound is made; the chain is broken."[8] But mind does more than struggle: it produces the novel. "How can we distinguish the force of mind, if it exists, from other forces save in this, that it has the faculty of drawing from itself more than it contains."[9] "Notre volonté fait déjà ce miracle. Toute œuvre humaine qui renferme une part d'invention, tout acte volontaire qui renferme une part de liberté, tout mouvement d'un organisme qui manifeste de la spontanéité, apporte quelque chose nouveau dans le monde."[10]

There is no doubt that, for Bergson, *creation* is the primary and the chief function of mind. The dynamical principle in consciousness, however, also *selects* and *organizes*. In both forms of expression of the *élan vital,* instinct and intelligence, materials used by the creative energy are selected to a given end and also organized. "L'instinct achevé est une faculté d'utiliser et même de construire des instruments organisés; l'intelligence achevée

[8] Bergson, H., *Mind-energy; lectures and essays,* N. Y., 1920, pp. 25, 26.

[9] *Ibid.,* 27; cf. *L'évolution créatrice,* 4th ed., Paris, 1908, p. 273; *Time and free will: an essay on the immediate data of consciousness,* London, 1910, 140-143.

[10] *L'évolution créatrice,* 260.

est la faculté de fabriquer et d'employer des instruments inorganisés."[11] Especially as regards intelligence, Bergson explains that "la fonction essentielle . . . sera donc de demêler, dans des circonstances quelconques, le moyen de se tirer d'affaire. Elle cherchera ce qui peut le mieux servir, c'est-à-dire s'insérer dans le cadre proposé."[12]

Bergson's relation to vitalism has been much discussed. Here it calls for only a comment. To "mechanism" the philosopher of creative evolution opposes "dynamism," a principle which is allied to vitalism so soon as it suggests a creative force in life. Dynamism is, however, more ambitious than most forms of vitalistic doctrine because it is based upon a dialectic of mind and matter. This dialectic leads it straight toward the problems of knowledge and reality. It seeks, that is to say, to interpret the whole universe in terms of vital,—*i.e.,* conscious,—creation.[13]

Sigmund Freud

(Annette Baron)

The dynamical principles of Freud are of the same general character as those which we have found in Janet and Bergson. The main emphasis, to be sure, is differently placed. Bergson's chief force is the vital principle, Janet's the personal consciousness, while Freud's may be said to be the "vital, personal unconscious." The main spring of Freud's unconscious is the vital impulse, the *libido,* a force which virtually becomes personalized, even personified, under repression. So are performed the tasks of selecting, condensing, translating, symbolizing and censoring. "The unconscious is the real psychic. . . . It must be accepted as the general basis of the psychic life. . . . Everything psychic exists as unconscious."[14] This "real psychic" Freud constantly describes in terms of "psychic force," "psychic energy," and

[11] *Ibid,* 152.

[12] *Ibid.,* 163.

[13] A competent and informing review and critcism of the psychological implications of vitalism, in its various classical forms, may be found in H. C. Warren's article "Mechanism versus vitalism, in the domain of psychology," *Philos. Rev.,* 1918, xxvii, 597-615.

[14] Freud, S., *The interpretation of dreams,* N. Y., 1913, 486, 487.

"psychic effort." The dynamic characteristics are applied to two "kinds" or "systems" of the unconscious; (1) to the "Unc. system" where wishes press for fulfillment, and (2) to the "Forec. system," which stands like a "screen" or censor between the exigent wish and its conscious discharge. The second system not only "bars access to consciousness" but also controls bodily movement and emits "mobile energy," a part of which is attention.[15]

Thus we find in Freud an exceedingly elaborate and exceedingly hypothetical account of mind written in terms of force. The account was first designed for the understanding and the relief of certain mental disorders; but later it was variously applied to dreams, humor, myth, lapses of speech and thought, the origin and development of society, and the springs of human action. As regards the use of dynamic agencies, Freud's system lays more stress upon the conflict of forces[16] than we find in the expositions of the French philosophers and physicians. The minds of Janet and Bergson, *e.g.*, are, as we have seen, essentially constructive; while Freud's mind is set against itself. It is torn by strife between the individual and society. In the great works of genius, to be sure, mind is creative; but even here creation appears as a release, almost a by-product, of conflict.

William McDougall

(Carl Rahn)

When we turn from Janet and Bergson to McDougall[17] to discover in what respects his conception of "soul" partakes of a "dynamic" character, we note that his notion of mental activity developes in a fairly definite way toward "creation" and toward "organization." By applying the method of immanent criticism to his presentation, we find that there runs throughout an im-

[15] *Ibid.*, 488.

[16] "We explain it [the psychic fission] dynamically by the conflict of opposing mental forces, we recognize in it the result of an active striving of each mental complex against the other." *Amer. J. of Psychol.*, 1910, xxi, 194. Cf. *Vorlesungen zur Einführung in die Psychoanalyse,* Leipzig und Wien, 1918, 64.

[17] McDougall, W., *Body and mind; a history and a defense of animism,* New York, 1911.

plicit distinction between the physical conditions for the occurrence, rise and subsidence of mental processes and the *physical correlates* of the same processes. As to the physical *conditions,* McDougall implies that it is when bodily processes come to a "stasis" that the "corresponding" meanings, belonging to the psychical order, arise as conscious perception, thought, and striving. As to the physical *correlates,* we learn that "meaning has no immediate physical correlate in the brain that could serve as its substitute and discharge its functions." Meanings are "products in consciousness of a purely psychical activity" (311), and they are the factors which awaken within us the appropriate emotions and psychical impulses or conations.

Under the *condition,* then, of stasis there becomes operative a "psychical activity" that has no immediate physical *correlates;* and the "products" of this psychical activity are meanings, values and conations. It is in this sense that mind is "creative" for McDougall. This creative activity is conceived to operate in accordance with "psychical dispositions that have been built up in the course of the experience of the race." Being built up, they determine further development and—may we infer?—would thus constitute, collectively, a psychical entelechy with an historical development.

With respect to McDougall's conception of mind as organizer, we find first, that the soul is invoked, after the manner of Lotze, because "the fact of the unity of consciousness correlated with the physical manifold of brain-processes cannot be rendered intelligible without the postulation of some ground of unity other than the brain or material organism" (356). As organizer, the function of the soul is two-fold: (1) it gives unity to the manifold of sensation-processes and (2) it "plays an essential rôle in the building up of the organization of the brain" (279). Concerning (1), the organization of sensations, it is simply to be noted that the independent sensory processes are unified by their integration into "meaning," which is the product of "psychical activity." Concerning the manner in which (2), the "organization of the brain," is affected by psychical activity, we are told that "the product of this psychical activity" (the meanings)

"stirs up" the psychic impulses or conation "without which no action is initiated or sustained" (311). The facts, then, are said to point directly toward the view that conation or psychical effort really intervenes in the course of the psychical processes of the brain. And it may be plausibly maintained that all other modes of consciousness serve but to guide or to determine the incidence of conation, the primary and most fundamental form of psychical activity. This organizing of physical processes in the brain by psychical conations is conceived to be a process of "guidance without work," consisting essentially in a "concentration of nervous energy from places of low potential into one system of neurons where the potential is raised to a high level (278),—though McDougall admits that the concentration occurs "in a way which we cannot clearly define."

III.

We find, then, dynamical principles conceived and applied in a variety of ways in current psychologies. For Bergson and for McDougall mind is a creator; for James and for Woodworth it initiates and it drives toward a conclusion. The conception of mental powers held by James and by Bergson also includes choice and selection in a prominent way; while McDougall joins Janet in regarding organization and unification as essential functions of a dynamical mind. Freud's *libido* is also a driving, initiating force, to which is added the active government of thought and behavior by the repressing and selecting power of the censor. Mind, therefore, in our writers is active and dynamic in so far as it creates, initiates, organizes, selects and governs.[18]

When we consider the wide adoption throughout psychology of one or another of the forms and direction of force, as portrayed in these writings, we are led to acknowledge that dynamism has in our own generation exerted a profound influence upon the science. There seem to be two or three special reasons for the psychologist's recent appeal to force. The first lies in mental

[18] These same dynamical principles have also been used in systematic settings by Lotze, Brentano, Lipps, Stumpf, Fouillée and Wundt, and more casually by G. S. Hall, J. R. Angell, E. L. Thorndike, and many others.

pathology, which stood for a long time in a stagnating condition. Its neurological basis was well nigh sterile, and its psychology was jejune and unproductive. There is no doubt that its speculative use of mind as a theatre of forces, producing, organizing, and repressing, has injected into it new life. Its present state suggests the quickening in the sciences of life induced by the principle of natural selection. It may be that now, as then, a long period of speculative fervor will be followed by serious empirical studies. The next subject after medicine to profit from dynamism is the humanistic interpretation of the facts of mind. A unitary, enduring and creative mind has always—since its "discovery"—been a solace to mankind. McDougall's eager support of the psychical researchers and Bergson's sanction of moral "freedom" are significant indications. Neither can we pass over the interest of the student of behavior in the fruits of dynamism. There is no doubt that an account of bodily performance, whether of the animal or of man, of the child or of the adult, of the normal or of the deranged, is rendered vastly more simple and convincing by the admission of a principle of creation, organization and direction. Much that would otherwise fall to observation and to logic is more elegantly done by a force or power. "Instincts," "capacities," and "intelligences" can thus be hypostatized and made to serve as surrogates for facts. The temptation toward dynamism in psychology bears more than a superficial resemblance to the lure of vitalistic "causes" in biology. It is the same temptation under slightly different guises. It is, however, curious to observe that it has become especially attractive in both forms just at the moment when physics is inclined to reject dynamical concepts.

To separate fact and hypothesis is not always easy. I believe, however, that it is possible to distinguish, in a general way, those properties and offices of mind which are subject to observation from our explanatory principles which involve force and which are causally used to account for the facts.

Let us start with the psychology of process,—a persistent attempt to discover by direct inspection mental factors in the flux of experience. This kind of inspection has, as it appears to me,

brought to light four significant facts regarding the constitution and the offices of mind. (1) There are *mental processes* which are amenable to description, to arrangement in systems, and to classification. They belong to a unique order, not reducible to the objects and processes of physics and physiology. Again (2) these processes *stand organized* in unique constellations, and they follow each other in sequences which are not duplicated in the physical orders. (3) The organized processes *carry a meaning, i.e.,* they make reference to existence of other orders. As McDougall says, they are in this aspect unique. (4) These organized processes which bear meanings do, when conjoined with bodily processes, *accomplish various ends,* they *operate* in various directions, they *fulfil functions.*

So far, as I think, we are on the level of empirical observation; and so far we seem to stand in no need of any dynamic principle to be imported into mind. For the accomplishment, the operation, the function, whether it be the production of knowledge, the preservation of life, internal or environmental adjustment, or the valuation of objects and of conduct, the only contribution which mind is observed to make is meaning,—memorial meaning or perceptual meaning, existence meaning, or value meaning, anticipation or reflection. If the accomplishment is to be regarded as a function or a performance, then it is always, as it appears, a *psychosomatic function,* a performance which involves bodily terms and mental terms. The term "mental" function (unless it refers to the fact of meaning) seems to me to be without significance. Nothing like energy is observed in mental processes or in their organization; and meaning belongs to a wholly different category from force. On the other hand, the nervous system and the muscles are obviously designed for the reception, concentration, storage, and discharge of energy. If dynamical factors are required for the explanation of the facts of growth, derangement and behavior, the bodily parts and processes involved in the psychosomatic functions would seem to offer the appropriate vehicle for an active or dynamical cause.

The dynamists are obviously right in their contention that

mind is centrally and essentially concerned in disease, in organic performance and adjustment, and in the development of the individual and of the race. But to say that it is *dynamically* involved, is to go beyond the facts. The contribution of meaning or of reference appears to me to be the great and unique contribution of mind, the contribution which makes the psychosomatic functions different both in kind and in range from the physiological performances of the body.

At any rate, if a mental force is to be postulated and is to be used to explain the observations of the pathologist and of the student of behavior, it should be recognized that such a postulate is a sheer hypothesis, proposed only for the temporary purposes of explanation. So Freud seems to regard his elaborate "unconscious." To look upon such a principle phenomenologically would be like looking into the ether for the lines of force of a magnetic field or for elastic fingers reaching out from the sun to hold the earth in the clutches of gravity. This confusion of hypothesis with observed and verifiable fact is extremely common within psychology today. It has led to an illegitimate substitution of forces and faculties for the empirical existences of mind and in so doing it has impaired the methodology of the science.

SOME NEGLECTED ASPECTS OF A HISTORY OF PSYCHOLOGY

By Coleman R. Griffith

Psychology stands in a peculiar relation to the sciences of life and to the physical sciences, for it is one of the youngest of Philosophy's children and, on that account, has fallen heir, as do the successive members of any growing family, to a number of family treasures, some good, some bad, and some indifferent. Among other things, it has inherited from the physical sciences a well-rounded methodology and a refined laboratory technique; and from the sciences of life, a "genetic" way of regarding mind in its relation to life. Moreover, in the near future, some one will write a history of the development of scientific concepts and it will then be discovered that psychology has fallen heir, also, to scientific ways of regarding the world at large, ways that became established a hundred years or so before mind was brought into the laboratory.

Now when psychology began to use these methodological and other heritages from the physical and the biological sciences in an attempt to understand mind, the opportunities for research became so great and the problems so insistent that the investigators of mind have been urged on to the present movement by nothing save the enchantment of their own productivity. Psychology became, over night, a realm of laboratory adventure. And within forty years of the founding of the first laboratory, general science is presented with the spectacle of a discipline whose facts already extend beyond the compass of encyclopedic volumes.

So rapid, in fact, has been the growth of the science, and so absorbing are its demands for the immediate future, that an adequate account of its genesis, a serious historical survey of the path by which it has come, is not a part of its immediate program. The psychologist who is at all historically-minded,

when trying to gain a perspective in his science, finds himself in the peculiar position of the man who wakes in a strange place and endeavors to comprehend his situation by taking a careful inventory of the furnishings of his room. If the science has come, by virtue of its achievements, to maturity, it must begin to realize that even scientific adventure is hedged about with historical restrictions to be understood and accepted before the adventure itself becomes of real significance.[1] Psychology is not merely the accumulation of fact in monthly journals; it is rather a product of the liberal past and a starting point for a productive future. The historian of psychology must tell us what psychology is, in its largest aspects, by telling us whence its methods and concepts have come and what these mean for its further development.

We have, at the present time, no history of psychology. That is to say, there is no written record of the genesis and development of the discipline as it stands.[2] Our historical researches are limited to the introductory pages of doctoral theses and other major pieces of research.[3] There are, of course, histories of

[1] An expanding interest in the history of science, in this country as well as in Europe, is a token of growth and maturity. It may be traced in the following papers: *Science,* 1915, *41,* 358-360; 1915, *42,* 746-760; 1919, *49,* 330-331; 1919, *49,* 447-448; 1919, *49,* 497; 1919, *49,* 66-68; 1920, *52,* 496; 1920, *52,* 559,562; 1921, *53,* 122; 1921, *53,* 163-164; 1921, *53,* 257-258. That the movement is being taken seriously is further shown by several papers appearing in *The Scientific Monthly.* See, *e.g.,* Gregory, H. E., History of geology, *The Sci. Mo.,* 1921, *12,* 97-126; Woodruff, L. L., History of biology, *ibid.,* 289-309; Bumstead, H. R., The history of physics, *ibid.,* 289-309; Brown, E. W., The history of mathematics, *ibid.,* 385-413.

[2] We are, of course, using the term "history" in the sense of a written account and not by way of reference to the *events* of which an account can be written.

[3] A very few illustrations from a single source will show the temper of such historical surveys; Sharp, S. E., Individual psychology: a study in psychological method, *Amer. J. of Psychol.,* 1899, *10,* 1-20; Whipple, G. M., An analytic study of the memory image, etc., *ibid.,* 1901, *12,* 409ff; Murray, E., A qualitative analysis of tickling, *ibid.,* 1908, *19,* 320ff; Geissler, L. R., The measurement of attention, *ibid.,* 1909, *20,* 473-502; Ruckmick, C. A., The rôle of kinaesthesis in the perception of rhythm, *ibid.,* 1913, *24,* 305-314; Boring, E. G., The sensations of the alimentary canal, *ibid.,* 1915, *26,* 2-5; Dallenbach, K. M., The history and derivation of the word "Function" as a

philosophy in which the central theme is mind or sense-perception or mental activity. One cannot neglect such accounts and neither can one forget altogether certain other historical surveys of the life and work of men who may, under protest, be designated psychologists; but the discipline has at present no searching and sympathetic survey of the events and ways of thinking that have led up to and that have grown into our present conception of psychology, its problems, and its methods.

Of 254 pages which Dessoir[4] devotes to an outline of the history of psychology, 148 describe events prior to 1800, and the remainder of the book barely takes us beyond Herbart. Brett[5] gets as far as Fechner at the end of 900 pages. Villa[6] devotes some 50 pages to a diary of the 17th, 18th and 19th centuries, while the rest of the book,—nearly 350 pages,—draws upon the history of philosophy for a large part of its discussion. The first volume of Baldwin's[7] history goes as far as Hobbes, while the second volume barely enters into the days of "mental chronometry" and the "James-Lange theory." Klemm[8] has done better than some of the others; but even so close a follower of Wundt as he has hardly entered into modern psychology and then has looked back by way of retrospection. Aside from the Wundtian bias, Klemm has written useful prolegomena to a history of psychology.

Histories grow and the past changes as it is seen in the light of new achievements. The cosmopolitan interest of the present-day psychologist is evidence enough that the science is broader than any existing account of its origin and of its growth. Who, for example, would attempt to describe, in advance of serious his-

systematic term in psychology, *ibid.*, 1915, 26, 473-484; Woods, E. L., An experimental analysis of the process of recognizing, *ibid.*, 1915, *26*, 314-317; Rogers, A. S., An analytic study of visual perception, *ibid.*, 1917, 28, 519-538.

[4] Dessoir, M., *Outlines of the history of psychology,* (tr., D. Fisher), 1912.

[5] Brett, G. S., *A history of psychology, ancient and patristic,* 3 vols., London, 1912-1921.

[6] Villa, G., *Contemporary psychology,* (tr., H. Manacorda), London, 1903.

[7] Baldwin, J. M., *History of psychology,* 2 vols., London, 1913.

[8] Klemm, O., *A history of psychology,* (tr., C. Wilm and R. Pintner), New York, 1914.

torical research, the mental and physical matrix which has stamped in so curious a fashion that current explanatory psychology known as Freudianism? Of the ways of regarding the social mind, of behaviorism and functionalism, of the renewed interest in a "psychology of the soul," and of scores of small problems, we are inclined to speak retrospectively with a superficial knowledge only of the historical facts. In all of these matters, we frequently refer, of course, by way of historical perspective to men and to events; but we have, as yet, no historical research in the field at large comparable in spirit to a recent small but choice example from Titchener.[9]

If, as we have already said, the science is to assume the responsibilities of maturity, it must turn seriously to its history, for such a quest usually tempers the ardor of youth but at the same time saves from stolidity. Now, if there is to be a history of psychology and if we are to speak intelligently of it, we must know upon what principles it is to be established. What significance, for example, will a different interpretation of the two terms "psychology" and "history" have for any statement of the problem and course of historical research in the mental sciences? Obviously, a history adequate to the science need not be three-fourths philosophy and neither must it be a history for purposes of propaganda in favor of any particular school of psychologists.

What, then, do we mean by "history" and by "psychology"? Let us first come to terms with "history." We are not here proposing a philosophy of history and we shall, therefore, be brief and somewhat schematic in describing at least two possible conceptions of the nature of history.[10] The first, which is the more venerable of the two, maintains that history is a chronological account of all that has occurred. That is to say, men sit down and, assuming a temporal sequence, proceed to write

[9] Titchener, E. B., Bretano and Wundt: Empirical and experimental psychology, *Amer. J. of Psychol.*, 1921, *32*, 108-120.

[10] We do not care to have it appear that we are dealing lightly or too naïvely with a question that has for years vexed the historian. He is, apparently, as sensitive to a statement of the problem of history as the psychologist is to a statement of the problem of psychology. This part of the science of history can be traced in the various historical journals.

a diary of the affairs of the world. Such an account proceeds as if one stood at the end of a street and described the succession of houses in adjoining blocks. An example of this static sort of description is taken from a current history of Italy:[11] "Robert the Wise (of Anjou) (1309-1343), the successor of Charles II of Naples, and the champion of the Guelphs, could not extend his power over Sicily where Frederick II (1296-1337) the son of Peter of Aragon, reigned. Robert's grand-daughter, Joan I, after a career of crime and misfortune, was strangled in prison by Charles Durazzo, the last male descendant of the house of Anjou in lower Italy (1382) who seized the government. Joan II, the last heir of Durazzo (1414-1435), first adopted Alfonso V, of Aragon, and then Louis III, of Anjou, and his brother René. Alfonso, who inherited the crown of Sicily, united both kingdoms (1435), after a war with René and the Visconti of Milan."

It has been urged that this method is the only scientific method of dealing with historical data. Static historians point out that interpretation and elaboration in history are as open to objections as is interpretation in any of the sciences. As a matter of fact, the static method does escape the great danger besetting the second conception of history, the conception that history consists mainly of an exegesis or an expounding of discrete facts in the light of some ligating principle or principles. In this respect, history as we look back upon it seems to be an unfolding, an efflorescence, an explication, providing we can use such terms without implying teleology. It is a curious fact that this second sort of history, genetic history, was largely supported by the biological sciences. Within a decade men began to realize that the whole earth and everything in it had a history, a genesis, a growth, an evolution. They realized that only a part of the story had been told by their static description of events. The main problem *had been* to state history "wie es eigentlich gewesen." The genetic point of view made as its quest history "wie es eigentlich geworden."[12] This second type of history,

[11] Quoted by Robinson, S. N., *The new history,* 1912, p. 3.

[12] Robinson, S. N., *op. cit.,* p. 78.

then, involves a high degree of ligation between facts, and the quest of the geneticist is directed primarily toward the principles of ligation, the bonds that give his bare temporal successions a unity and onward-moving significance.

There are, then, at least two ways of regarding history. The historian can be "ultra-scientific"; that is to say, merely descriptive or static, and so put down his facts in orderly temporal succession; or he can enrich and enliven his account by reading into them the culmination of tendencies, the inception of movements, the mental-like stream of pregnant and forward-tending events.

Let us turn now for a moment to psychology. The answer to our query: How are we to write a history of psychology? depends quite as fully upon the meanings of the word psychology as upon those of history. For our present purposes we can distinguish two meanings of the word. In the first sense and at the same time the broadest sense psychology refers, in a general way, to all the events or facts issuing from the existence in the world of minds or of anything mental. That is to say, psychology is a blanket term to cover almost anything from the alleged appearance of dead friends or of the latest achievement of a superior dog to an abstruse discussion of the problem of knowing or of the immortality of the soul. In the second place, psychology may be defined rigidly so as to include only a scientific description of mind, of mental activity, or of mental products. There have arisen, of late, a number of such statements regarding the nature of psychology, statements that definitely exclude a large amount of material popularly known as mental. Moreover, many of these recent descriptions of psychology take the psychologist farther away than ever from certain borderland problems which have in the popular mind formed the central province of psychology. Finally, scientific accounts of mind have eliminated a large number of philosophical problems concerning the nature of mind, of knowing, of the reliability of sense-perception, the origin and significance of the self, and so on.

With these distinctions before us, then, can we state the rele-

vant features of a history of psychology? Our answer must fall under four headings. If history is to be regarded as a *diary* and psychology as the accumulation of common sense and reflective thinking, we shall derive an account considerably at variance with the account issuing from a chronological description of psychology as a science. On the other hand, if history is an interpretation of the diary, a genetic, dynamic account of a growing thing, our psychological history will depend upon our choice of psychology as a general popular discipline or as a specific scientific discipline.

Since much depends upon our choice of a method of writing our history, we shall briefly illustrate the kinds of history falling under each of these conditions. Suppose, for the moment, that history is a diary and that psychology is a general name for the study or observation of anything mental. Our historical chronicle would begin, then, with the first written records we have of man's dealings with mind, either by way of examination or of superstition or by way of reflection on the problem of knowledge. Such a history would take us back to the life and supposed work of Thales and then hasten us through the births and deaths and the date of the principal works of Pythagoras, Heraclitus, the Eleatics, Democritus, Plato and Aristotle, and finally, after many chapters, would give us a breathing spell among the chuch fathers. In these early chapters we should have become acquainted with the numerical relations of the typical Pythagorean, the cosmic and mental elements of Heraclitus or of Empedocles, the sieve-like theory of sense-perception from Democritus, the tripartite world of Plato or the realization of the potential in Aristotle. The chief emphasis in such an account, as is evidenced by most of the text-books on the history of psychology, is the contribution of each individual to a growing body of knowledge regarding mind and the world in which it lives. At least, the emphasis is certainly not on the spirit of the time or the factors in the lives of the men that made their contributions possible.

In writing the chapters on the psychological contributions of

the church fathers, some genetic reference must be made, at the least, to Plato and Aristotle. All that the patristic leaders did was in a large measure colored by the writings of Plato and Aristotle. But even so, the other factors that existed in the political and social conditions of the times find no part in a description of the conditions that made patristic psychology what it was. Our daily chronicle would take us through Marcus Aurelius, Tertullian, Origen, Plotinus, St. Augustine, Thomas Aquinas and Duns Scotus. The history of psychology, regarded from the point of view here considered would still mention births and deaths and the principal contributions of each to our knowledge of the functions of the soul and of the intellect and the respective importance of divinity and of the will in the control of conduct. Two hundred fifty years later we should suddenly find ourselves at the inception of a large empirical movement of which Bacon was the first representative and Hobbes and Locke worthy followers. These men formulated doctrines of the nature of mind and of the problem of knowledge that dominated English thought for nearly three centuries, but if the psychologist knows the origin and significance of this movement, it is by way of general history and general literature and not by way of historical research in psychology. In the meantime, under an impetus from Descartes in France and Locke in England, Malebranche crystallizes French thought for a short time and then the record becomes discontinuous, with a number of contending movements of different value. The whole of Germany falls under the spell of Kant and while the chapters of the text run on with the details of the diary, we suddenly find ourselves in the midst of a psychological laboratory with a conception of mind that is scientifically possible and with a method that begins to produce results with amazing rapidity, but with small appreciation of why we have arrived and where we may expect to go. The whole account from this point of view is just a chronological sequence, the noting of the appearance of new movements and of the men responsible for them.

This general situation is similar if we take history as a diary

and psychology as a science, save that our history of psychology begins, not with Thales or early Arabian thought, but with some such time as the publication of the *Beiträge zur Theorie der Sinneswahrnehmung* in 1862. The spirit of the account, however, is not different from that just given. The *Grundzüge* is followed by the *Tonpsychologie,* the *Zeitschrift für Völkerpsychologie und Sprachwissenschaft* by the *Völkerpsychologie,* while Hermann's *Handbuch* provides an appropriate sense physiology. The locus of the history is largely in Germany until the late years of the 19th century, when several of Wundt's pupils returned to this country, established their own laboratories and continued the scientific productivity of the German universities.

Now let us turn for a moment to the conception of history as interpretation and to psychology regarded first as accumulated common sense and secondly as science. In this second type of history a new spirit guides the account. We find that men have not only lived and contributed but that they have reflected; they have absorbed from their forefathers and from their contemporaries, and their work is alive with meaning and reference. Thales becomes the spokesman of his day and reflects the type of thought about him. The record from him to Aristotle is not discrete but continuous, and Aristotle is what he is because of the contributions of those who have lived before him. The task of the interpretative psychologist is to discover what there was in the lives of men and in the political and social organization of the time that made the contribution of Aristotle possible. The patristic psychologists are not isolated commentators on the functions of reason but they are rather the reflectors of profound religious, social and political tendencies in the lives of the people. In this history of psychology we do not find ourselves suddenly in the midst of an empirical movement but we find instead a number of tendencies leading for years toward the formulation of just the problems with which Bacon struggled. In the nineteenth century psychology does not come suddenly upon a scientific conception of mind and of psychological method. These are things that have come out of the lives and work of men

who were sometimes remote in place and in thought from the events to which they unwittingly contributed.

As is the case in the first type of history, the account is considerably shortened if psychology is considered a science. As a matter of fact, we have now come upon what seems to be a real basis for writing a history of psychology. From this point of view, we no longer need to take up half or three-fourths of our text with an account, either chronological or interpretative, of the problems that are essentially philosophical and not psychological. They become of significance and of interest only in so far as they furnish the basis for the interpretative account that must, for the sake of convenience, begin at some arbitrary date, as for example, the founding of the Leipzig laboratory. In this account of the history of psychology, Meumann and Külpe and Helmholtz and Stumpf and Ach and Messer and others do not stand apart from one another but they are creatures with unique historical backgrounds. The work that they did falls into order and assumes significance only in so far as it represents or reflects tendencies which have their roots in the past, some temporally near and some temporally far away.

In answer to our question, then, we can say that a significant history of psychology can be written with the most of it falling within the last fifty years and but little of it in the preceding twenty centuries. Ebbinghaus's remark that psychology has had a long past and a short history emphasizes the fact that the long past is only a mold in which were cast the essential features of the science. The past, that is, the past prior to some such arbitrary date as we have named, becomes significant only in so far as we need it to interpret the facts and tendencies with which we are now dealing.

If this conception of the history of psychology is acceptable, certain other aspects of the problem become immediately insistent. The histories we have at the present time are but prologomena to a real history. They are concerned, for the most part, with the pre-psychological facts. It is true that they can be supplemented by the introductory chapters of a good many

doctoral theses, but even so we come upon the fact that the science stands in need of much serious historical research, the results of which will form the body of theses instead of brief introductions. We have buried ourselves so deeply in our laboratories and dissipated our energies so prematurely in the fields of psychotechnics that we are, to judge from the tenor of a large part of current periodical literature, losing contact with the real problem of psychology. It is true that psychology has become a complicated discipline. We have said before that it includes a large number of tendencies. The histories that have usually led up to sensationalism have missed altogether the tendencies issuing in functionalism and behaviorism to say nothing of borderland groups of facts that, out of scientific fairness, must be taken into account. The historian of the science must, then, as soon as time reduces and properly valuates our facts, put them together in an organic whole and see what they mean as history. The science needs the impetus to healthy growth that comes from a knowledge of the contributing factors to its existence. One of the chief ways of properly estimating the overnight development of mushroom "psychologies" is to examine critically the kind of soil out of which they have appeared. All the world loves a good problem and a sound method, but neither of these comes from sterile ground. They are generally the results of long incubation or simmering, and their real value to the development of the discipine as a whole falls under the scrutiny of the historian who can place them in proper perspective.

The historian of the science of psychology stands in a peculiar relation to his fellow historians. He is dealing with material that is more like mind in its fluent character than any other process. The nature of mind is such that it must be viewed in the light of its own organization and function. The psychologist, by virtue of his knowledge of the subject-matter with which he deals, is peculiarly fitted to exhibit the conditions under which points of view develop and the ways in which our present achievements are related to the past. Mind in its own development is cumulative in a peculiar sense, and as the historian of mind views the facts of his science he finds that they too are, as

mental monuments or products of mind, also cumulative. By virtue of his training and his knowledge, the psychologist is committed to the type of historical research we are urging. He has stripped his science of its meaning if he contents himself with a chronology. He is bound to regard the past as a promise of the future and the present as the natural outgrowth of the past.

It appears, then, from our discussion thus far that a history of psychology should consist of an interpretative account of psychology taken as a science. This makes the historical account short in the time covered but long in the developments included. We have found also that the proper valuation of the field as it is rests first of all upon sound historical research, and that the psychologist was peculiarly fitted to be a historian.

We have now to ask what should be the central factors in a history of psychology? About what central theme should the account be written? Too often histories are colored by the desire to show that events are leading naturally to some favored system of interpretation of fact or are useful for purposes of propaganda. But, viewing the science in the large, is there a central theme about which the history can be written, a theme which will not at the same time be an excuse for propaganda. There are a number of possibilities appearing at once. Psychology has depended largely upon the formulation of its methods. But it has also developed a scientific statement of its problem and it has discovered certain vital relations with other scientific disciplines. Let us propose, however, in order to bring the matter to a focus, that the history of psychology should have as its central theme the tracing of the stages in the development of a scientific conception of mind. Method and problem have waited upon a conception of what the subject-matter of psychology really is. If the history of psychology means anything, it means that all that men have done or are now doing in the field rests essentially on this one problem, *viz.*, what is mind? This kind of difficulty did not materially hinder the development of the physical and the biological sciences; although they had to outgrow the belief that life was a manifestation of some immaterial force or power and that events in the

physical world were controlled, not by natural laws, but by resident spirits of one kind and another. The development of a scientific conception of mind, however, has been a serious problem. Indeed, it appears to be one of the most serious problems with which the history of psychology has to deal. If mind is a form of energy, then our method and the statement of our problem are to a certain extent already established. If mind is the manifestation of the soul in the body, other methods and other problems are presupposed.

Now, as a matter of fact, two generations of psychologists have been working on an empirical level with just such a concept. As a result of centuries of reflection, and by virtue of inheritances from the related sciences, psychology turns out to be neither the study of the activities of a soul nor the study of a subtle kind of energy. On the other hand, the science consists of an accumulating series of observations directed toward mental experiences. The laboratory has gone to work on the assumption that if it takes a small bit of human experience and repeats it over and over again under conditions that are carefully controlled and undertakes to reduce the experience to its smallest constituents, it has made á scientific description of the event. When all such experiences are thus scrutinized from the point of view of composition, of organization, and of function, and the facts are then moulded into a system, the task of the psychologist is done. His labors have been fruitful beyond his early anticipation.

It is obvious, then, that a history of the *science* of psychology must be written on the empirical level which the discipline has attained. A history of the functions of the soul or of the problem of knowledge or of the nature of the ego is no longer adequate to the mature dignity of the science. A substantial and adequate history will, according to our analysis, consist, in part, of a genetic account of the development of the scientific concept of mind, and, in part, of a survey of the products of the laboratory and the growth of empirically organized systems of psychology.

A PRELIMINARY STUDY OF THE EMOTIONS[1]

By C. A. Ruckmick

For several years we have made an attempt to investigate the emotions and, to some extent, the affective processes in general in accordance with the experimental procedure followed in connection with other processes. It has long been recognized that an empirical study of the affective dispositions was bound to meet with almost insuperable obstacles. For one thing, it was difficult to plan a series of experiments that would keep the observers naïve. If an observer has been once or twice tricked into an emotion, he will not only guard against a future repetition but he may actively set himself against the arousal or,—what amounts to the same thing,—he will not meet the attempt seriously. In another respect difficulties appear on the side of the process itself. If the charges of Herbart and of some of his predecessors concerning the distortion of the process under investigation have any weight, it is certain that they will have special significance in connection with the emotional experiences of the human mind.

It has been a common practice to attack the emotional life either from the side of its physiological expression or through its effects on other mental processes. A considerable amount of material is to be found in the literature concerning the physiological effects in the psychophysical organism, and latterly emphasis has been placed upon the secretions of the ductless glands. So far as we are aware, there has been very little work done

[1] The author is grateful first of all to Miss Merle Turner who posed for the photographs and eagerly took part in the long process of making, selecting, and often discarding the expressions obtained. Various divisions of the composite study were assigned to students who elected a major course in the Department of Psychology. Among them are Miss Harriet Anderson, Miss Marion Louise Smith, Miss Zeniar Kizer, and Miss Esther E. Kinsey. To this list ought to be added a long roll of observers. To all of these the author wishes hereby to give credit for work faithfully undertaken and done.

PLATE I

on the facial expression of emotion. The studies of Langfeld at the Harvard laboratories and some previous investigations of Feleky have brought once more to our attention what can be done with the human face.

I

The collections[2] of facial expressions so far published and available for general use are made up of line drawings of a heavily bearded face that was obviously "touched up" by some artist. Outside of the Columbia studies later to be mentioned, little or nothing had been done with the female face. We were curious, for example, to see what range of expression we could obtain without such accentuating accessories as a moustache and beard. We therefore arranged for a series of sittings with one of the talented women students in the University who had had considerable training in dramatic performances. The plan consisted in drawing up a list of expressions that we desired to photograph and in selecting for each day three or four of these for practice. During her spare time in the morning, the student would practice the expression before a mirror, frequently with the assistance of some classical quotation which she had recalled in connection with this particular emotion. She would then come at an appointed hour in the afternoon to one of the dark rooms in the laboratory which was illuminated by a high candle-power incandescent light. The camera was placed within three or four feet of the face and to one side, as close as possible to the camera, a mirror was hung in which she could observe her expression until the desired exposure was made. The experimenter, too, frequently criticized the expression, and sometimes, when the attempt proved to be unsuccessful, deferred that particular expression for a more opportune time. There was, of course, a suitable neutral background and the student was uniformly draped in black velvet so that all but the face fell into the background.

At this point it must be remarked that the expression of the

[2] Rudolph, H., *Der Ausdruck der Gemütsbewegungen des Menschen,* (2 vols.), Dresden, 1903.

emotion was necessarily confined to the face. The voice, for instance, played absolutely no part in the depiction of the emotion. In one or two instances only were the hands or other portions of the body permitted to take part and then merely to assist the portrayer in her efforts to express her emotion. Since this part of the body was not photographed, it could not enter into the interpretation of the expression.

The exposure was made for some five seconds and then both the student and the photographer repaired to the dark room to see whether the negative was satisfactory. If unsuccessful, that particular expression was repeated until it had been properly reproduced. Thirty-four negatives were retained when the process was completed. Examples of these expressions are shown in Plates I and II.

II

Our first attempts lay in the direction of determining how successfully the emotions were portrayed as judged from the interpretations independently made by over a score of observers. Some of these observers were left to their own choice of names, after being instructed to be as concise as possible. Other observers were given a condensed list of names from which to choose the corresponding photographic expression of the emotion, much in the same fashion as was done in the study of emotion at the Columbia laboratories.[3] The series of thirty-four photographs were submitted to each of the four observers with the following instructions:

"You will be shown the photograph of a face. Please note first what meaning you read into the face, and second, any change in your own affective reaction as a result of viewing the picture."

Each observer was asked at the beginning of the series to describe his emotional state. After viewing the picture, he was further instructed to write down (1) the name of a brief description of the emotion portrayed in the photograph and (2) a descriptive commentary upon his own inspection and interpretation of the picture.

[3] Feleky, A. M., Expression of emotions, *Psychol. Rev.*, 1914, xxi, 33-41.

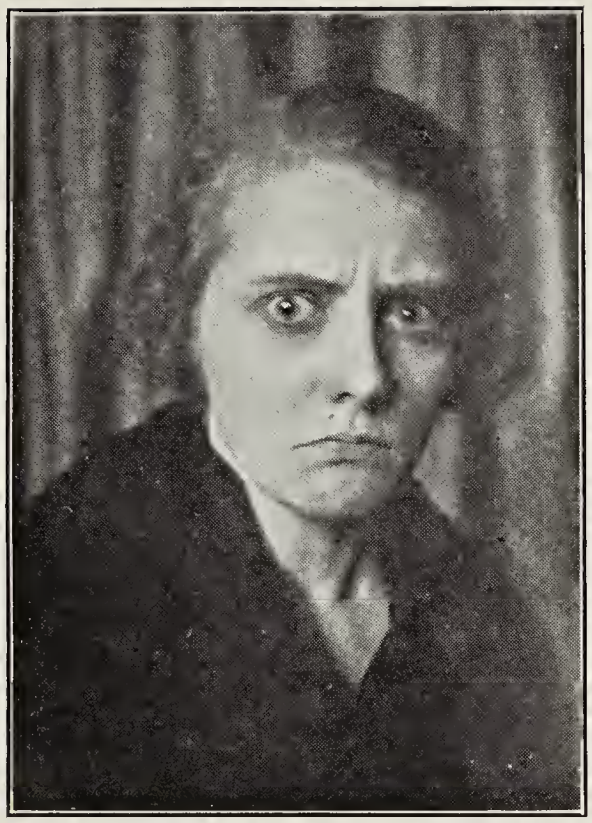


PLATE II

The attached list shows the variety of expressions used to designate the interpretation of the photograph. Some allowance must be made for inadequacy of verbal expression, as when an observer gropes about to find a word that will carry his meaning. Some emotional expressions, especially when the emotion becomes intellectualized, give a much less uniform response on the part of the observer than others, as will be seen by comparing, for example (see accompanying list), No. 14 or No. 20 with No. 21 or No. 30. As a rule. the "primary" emotions, as love and hate, joy and sorrow, are much more uniformly interpreted than the "secondary" ones, like repulsiveness, surprise, distrust and defiance.

In all these cases, the observers were asked to set down their interpretations without suggestions from the experimenter. The problem that suggested itself was the question regarding the rôle of the several facial features in suggesting the interpretation. Our photographs were so taken, as we have before noted, that the expression was confined to the face. The point now raised was: do the eyes furnish a better cue than the mouth, or the lines about the nose? In a preliminary study with some fifteen copies from the Rudolph collection, the face was divided up into three sections showing only the eyes in one case, the nose and lines about the nose in another, and the mouth in the third. In still another series the face was divided into upper and lower half. In this preliminary experiment it was found that next to the whole face the lower half of the face gave the best cues for interpretation; then in order came the eyes, the lower half, the mouth, and finally the nose and the lines about the nose. This series was repeated with our collection of thirty-five portraits with the same results. In order to avoid the observer's memory of the picture giving him the necessary suggestions, the series was so arranged that in each series of ten presentations each day one-half of the number were always new for that week. The results indicated, with only a few exceptions, that memory played comparatively little part in the interpretation of the partially covered face.

III

In still another experiment we attempted to show, on the basis of introspective evidence, what were the mental processes which conveyed the meaning of the portrait and to what extent there were individual differences in this procedure. We had casually noticed before, in previous studies, that some observers would visualize the entire person who portrait was shown and ideationally integrate the position of the hands, posture of the body, and occasionally relevant objects about the room; others could hear the words spoken; still others would call up memory pictures of their own similar experiences; and some would take the picture quite passively as a picture without going interpretatively behind it. There seemed to be in the last case comparatively little elaboration. The author has found the same difference in individuals attending cinematographic performances.

To illustrate the dramatization of these portraits and also the individual differences in the manner of their interpretation, a few statements from the observers will be quoted:

"She has been crying, but when another person enters the room she becomes defiant. She refuses to be dictated to. Hitherto she has been quiet but on the entrance of the second person she calls out angrily."

"The rather stary eyes give the idea of interest but of a person who is ill and has not physical strength or else is tired. I remember feeling the same way the subject looked when I have been tired."

"Someone for whom the girl feels responsible has gotten into difficulties and the subject of the picture is more or less terrified and searching anxiously for a solution of the problem."

"I noticed that my own eyes seemed to hurt when I looked at the picture and I felt a sagging of the muscles above the mouth."

"I feel a contraction of the muscles in the throat and tongue. My teeth are set and there is a feeling of strain."

"Terribly unpleasant visual perception of a drunken man and his children in the room."

IV

It appeared that the mood of the observer changing from day to day had some effect on the interpretation of the face. We

INTERPRETATIONS BY OBSERVERS

	Expression Intended	(Obs.) A	B	C	D	E	F	G	H	I	J
1	Bodily pain	Pain	Pain	Bodily distress	Suffering	Anguish	Pain	Pain	Bodily anguish	Despair	Pain
2	Resentment	Fear	Sadness	Surprise, resentment	Retrospection	Given up hope	Thought	Endurance	Hopeless suffering	Haughtiness	Despair
3	Scorn	Scorn	Contempt	Contempt, disdain	Disgust	Sneering	Scorn	Doubt	Disgusted scorn	Contempt	Disgust
4	Sulkiness	Disappointment	Dejection	Sullen, rebellion	Sulkiness	Moroseness	Sulky	Disappointment	Reproving	Poverty	Forlornness
5	Thoughtfulness	Contemplation	Thought	Angry resistance	Memories	Reproving	Attention	Interest	Thoughtfulness	Thinking	Defiant fear
6	Grief	Pleading	As one persecuted	Sorrow, hope of relief	Expectancy	Poverty-stricken	Pleading	Sight of pain	Dejected anxiety	Questioning	Plea for mercy
7	Amusement	Happiness	Amused	Humor	Welcome	Happiness	Contentment	Amusement	Cheerful, good humor	Happy	Care-free
8	Laughter	Joy	?	Gay laughter	Raillery	Joyousness	Merry	Enjoyment	Heartfelt joy	Amused	Joyousness
9	Interested amusement	Amused interest	?	Interested inquiry	Interest	Admiration	Happy	Entertainment	Snippy pleasantry	Expectant	Amused interest
10	Vindictive rage	Repulsion, anger	Anger	Gross fun	Fright	Insane, drugged	Hate	Terror	Maniacal fear	Contemptible	Evil
11	Surprise	Pleasant surprise	Surprise	Surprise, pleasant	Wonder	Surprise	Surprise	Enlightenment	Surprise and joy	Surprise	Surprise
12	Extreme anger	Disappointment	Discouraged	Surprise, apprehension	Apprehension	Intense thought	Dismay	Fright	Dread	Disappointed	Discouraged fear
13	Tearful sorrow	Sorrow	Disillusion	Stupefied grief	Dejection	Grief	Sorrow	Grief	Sorrow	Sorrowful	Grief
14	Terror (incl. fear)	Astonishment	Fear	Horror	Startled	Sudden fear	Astonishment	Horror	Fright	Startled	Fright
15	Horror (incl. suspense)	Pained surprise	Fear	Apprehension, painful sight	Agitation	Torture	Shock	Suspense	Bodily pain	Hurt	Horror
16	Hopelessness	Wistful	?	Rueful meditation	Sadness	Resigned-to-fate	Wistful	Pity	Longing	Dreaming	Pity
17	Worship	Inspired	?	Mild, passive elation	Hope	Spiritual	Inspiration	Awe	Peaceful contemplation	Adoration	Adoration
18	Apprehending an idea	Amused questioning	?	Suspended anguish	Interest	Oh!	Questioning	Inspiration	Quizzical contemplation	Whimsical	Cynical interest
19	Interested observation	Love	?	Pleasing observation	Attention	Interest	Interest	Convinced	Interested observation	Pleasant	Superiority
20	Anger	Angered fright	Hate	Anger	Vindictiveness	Anger	Anger	Disagreement	Excited	Angry	Hate
21	Distrustful hate	Distrust	Mean	Critical disapproval	Sulkiness	Peevishness	Distrust	Dissatisfaction	Earnest critical superiority	Scorn	Rebellion
22	Begging entreaty	Pleading	?	Sentimental admiration	Mild resignation	Assumed happiness	Pleading	Wistful	Pathetic, appealing	Slight pleading	Don't care
23	Distrust	Suspicion	Distrust	Critical reserve	Doubt	Distrust	Doubtful	Suspicion	Keen, questioning	Revengeful	Sneaking deceit
24	Dumb surprise	Wonder	Incredulous	Wonder and fascination	Surprise	Disputing air	Wonder	Revelation	Surprised fear	Astonishment	Astonishment
25	Haughtiness	Haughtiness	Firm	Rebuke	Haughtiness	Haughtiness	Haughty	Defiance	Superiority	Haughty	Pride
26	Defiance	Angry defiance	Defiant	Repressed inquiry	Reproach	Stubborness	Defiant	Challenge	Commandeerng snobbishness	Indignant	Defiance
27	Curiosity	Interest	?	Rapturous contemplation	Faith	Look of inquiry	Attention	Convincement	Interested, non-hearted	Interested	Love
28	Sympathy	Sympathy	?	Mild, wistful rebuke	Indifference	Pleading	Sympathy	Wonderment	Hurt surprise	Coaxing	Pleading
29	Screaming pain	Anguish	?	Agony	Hysteria	Terrible suffering	Anguish	Past endurance	Great bodily anguish	Pain	Fear and pain
30	Contemptuous smile	Cynical humor	?	Mischievous	Amusement and contempt	Worldliness	Sarcastic	Mild, forced interest	Foolish, simple-minded gaiety	Leering	Sneering
31	Suffering, not bodily, pain	Ill, faint	Surprise	Dull, pain	Pain	Tired, pain	Intense pain	Agony	Hopeless pain	Wounded	Pain
32	Tearful laughter	Sympathetic love	?	Mild gaiety	Reconciliation	Content	Love	Relief	"After sorrow comes joy"	Yearning	Frivolous
33	Violent laughter	Mirth	?	Explosive enjoyment	Amusement	Joking	Mirth	Happiness	Vivacious youthfulness	Laughing	Joy
34	Shocked surprise	Surprise	Surprise	Consternation	Horrified	Astonishment	Awe	Awe	?	?	?
35	Skeptical smile										

undertook therefore another series of investigations using four observers of markedly different temperaments, depending in some instances on daily changes in their moods and in other instances contriving means, either by showing unusual pictures or by making various statements to them, to change from time to time their affective disposition while looking at the pictures. For instance, photographs of starving Viennese children were available at the time. These were shown quite suddenly to observers, and the effect on their subsequent judgment of the facial expressions was noted as compared with normal series. Introspections revealed whether or not the desired effect was actually experienced. On another occasion, the observer would be scolded in the presence of others, and sometimes by the instructor, for being habitually late to his appointments or for failing to hand in required work.

Series were obtained also in order to ascertain variations in the interpretations, made from day to day, that were due to other than emotional disturbances. Our results show, in agreement with Langfeld,[4] that individuals vary not only with regard to suggestions from without but also in accordance with emotional experiences often concealed from another in interpreting the same facial expression on different occasions. Three of our observers allowed these experiences to color their judgment; one observer was apparently immune to all influences even of a highly emotional nature by way of changing his judgment. Often the change in interpretation is slight, as, for instance "pain" on one occasion, and "agony" on the other, or "amazement" at one time and "horror" at another.

In going over our detailed accounts, it is noticeable that fewer changes are made under suggestion in those emotions that are usually classed as "primary"; while more radical changes of judgment occur in those that are not so easily named.

[4] Langfeld, H. S., Judgments of emotions from facial expressions, *J. of Abnormal Psychol.,* 1918, xiii, 172-184; Judgment of facial expression and suggestion, *Psychol. Rev.,* 1918, xxv, 448-494.

A COMMENT UPON THE PSYCHOLOGY OF THE AUDIENCE

By Coleman R. Griffith

A group of university students registered in a given course and assembled for the lecture-hour displays in a unique manner the typical characteristics of an audience of the polarized sort.[1] The chief social relations obtaining between the speaker and such an audience are of the all-to-one and one-to-all types.[2] The auditors, so far as general dispositions are concerned, are in a receptive, expectant mood, with a homogeneity of interest which serves to carry the lecture-topic at a fairly high level of attention. Such an audience is characterized by a sophisticated attitude that eliminates the distress and disturbance of unfamiliarity; by a general cortical set which determines in advance of the lecture the general apprehensive and affective patterns; by a preliminary tuning induced by familiar faces, significant apparatus, drawings, charts and so on; and finally, by associative tendencies which focus the attention of the group upon a single topic. It is under these conditions that the all-to-one and one-to-all relations develop; but it is not at all evident that the bonds from every part of an audience are equally effective in drawing individual members into social contact with the speaker. There is no reason to suppose that, in the all-to-one relations, an individual in the periphery of the group is as definitely polarized as individuals nearer the speaker. It might, on the other hand, be conjectured that such an individual was much less an integral part of the group, for the perceptual and affective patterns from nearby neighbors must certainly contribute to the maintenance of the all-to-one relations. Frequently an outlying member of a group does not have just these clues to social integration and the lack may, as

[1] See Bentley, M., A preface to social psychology, *Psychol. Monog.,* 1916, xcii, (vol. *21*, No. 4), 20ff; Woolbert, C. H., The audience, *ibid.,* pp. 37-54.

[2] Woolbert, C. H., *op. cit.,* pp. 44ff.

has been recently suggested,[3] make a difference in the level of performance of such members of the group.

On the other hand, it is a common complaint among students who sit at the rear of large lecture-rooms that they can neither hear the lecturer nor see his demonstrations. If this complaint is well founded, the fact ought to be reflected in the accomplishment of such students. Now academic grades are a measure of accomplishment in both of the situations we have mentioned, and it follows that a critical analysis of the distribution of such grades in lecture-halls ought to betray differences in performance that cannot be attributed to differences, either in mental ability or in physical well-being.[4]

If it can be determined, other things being constant, that the performance of students at the rear of a large room is actually less than the performance of those at or near the front, the students are partially justified in their complaint. We are not here interested in the complaint, however; instead, our inquiry touches the question as to whether a difference does actually exist between grades in different parts of a room, and if it does, for what reasons.

The answer to our query was sought by a tabulation, according to the seat-numbers in five large audience-rooms at the University of Illinois, of the grades of students registered in several large courses. In every case the students were alphabetically seated. Mid-semestral, class, laboratory and final examination grades, as well as the final semestral grades, were considered. Courses were sought which, at the one extreme, were conducted with the

[3] For example, Allport (Allport, F. H., The influence of the group upon association and thought, *J. of Exper. Psychol.*, 1920, *3*, 159-182) has shown that performance in association and thought depends to a certain extent upon social factors.

[4] Recent experimental education has, as we all know, been deeply concerned with differences of performance which parallel differences of mental ability. A great deal of attention has been given also to the influence of health and of other physical factors on the degree of performance. We are not here concerned with intelligence ratings, however, but are assuming that mental ability, physical well-being, and similar factors are evenly distributed, under the conditions which we have chosen, throughout a given auditorium.

minimal amount of lecturing and a maximal amount of quiz and laboratory, and, at the other extreme, were conducted with a maximal amount of lectures and a minimal amount of quiz and laboratory work.

Now the tabulation of something like twenty thousand such

TABLE I

Showing 1st and 2nd Quiz grades, final examination grade, and final semestral grade of all students in Course——, 2nd semester, 1916-1917.

ROWS	GRADES	THE OCCUPIED SEATS	AVER.	M.V.	AVER. (ALL GRDS.)	AV. OF M.V.
8	1st Quiz	18 80 60 51	52.2	17.7	60.1	12.6
	2nd Quiz	50 77 60 65	63.0	8.0		
	Final Ex.	53 81 79 50	65.7	14.2		
	Final	60 78 75 50	65.7	10.7		
7	1st Quiz	77 44 94 36 71 75 48 69 32 79	62.5	18.1	75.6	12.0
	2nd Quiz	83 90 98 51 91 76 75 74 55 93	78.6	12.4		
	Final Ex.	85 79 95 85 99 84 73 84 39 86	80.9	10.3		
	Final	86 81 91 71 90 87 76 78 60 86	80.6	7.3		
6	1st Quiz	40 69 61 50 93 84 79 42 71	65.4	15.6	76.8	10.0
	2nd Quiz	66 90 73 83 95 85 84 64 75	79.4	8.8		
	Final Ex.	80 89 81 61 98 81 96 83 80	83.2	8.2		
	Final	65 80 78 72 92 83 87 71 82	79.0	7.4		
5	1st Quiz	69 67 93 69 44 55 92	69.8	12.9	75.9	8.1
	2nd Quiz	74 76 84 71 51 78 89	74.7	8.0		
	Final Ex.	77 83 88 80 80 64 89	80.1	5.6		
	Final	75 80 87 77 70 73 91	79.0	6.0		
4	1st Quiz	65 66 95 37 82 69 80 61 76	70.1	9.5	78.8	7.1
	2nd Quiz	70 76 84 59 76 75 95 70 99	78.2	8.1		
	Final Ex.	81 90 93 77 85 86 93 73 93	85.7	6.1		
	Final	80 82 86 72 82 80 98 73 90	82.5	5.7		
3	1st Quiz	72 94 63 60 70 69 89 56	71.6	10.7	76.0	6.3
	2nd Quiz	59 80 77 77 82 80 80 71	75.7	5.4		
	Final Ex.	73 85 87 85 79 68 73 90	80.0	6.7		
	Final	73 78 80 74 80 75 75 78	76.6	2.4		
2	1st Quiz	83 35 80 56 60 89 88 62 66 66	68.5	13.2	74.0	9.5
	2nd Quiz	68 55 55 61 60 96 71 63 88 60	67.7	11.5		
	Final Ex.	70 58 79 85 88 94 90 85 90 85	82.4	7.9		
	Final	75 65 78 74 71 92 84 77 85 73	77.4	5.3		
1	1st Quiz	81 88 62 69 86 95 30 21	66.5	21.6	72.3	12.8
	2nd Quiz	76 67 58 69 74 98 52 46	67.5	10.5		
	Final Ex.	96 86 71 54 76 91 85 73	79.0	10.5		
	Final	86 86 70 65 77 93 71 62	76.2	8.6		

grades does make it evident that, with the significant exceptions to be noted below, the accomplishment of students in the front rows is from 3% to 8% less than that of students in the middle of the room. The results indicate, further, that there is a still more marked difference,—approximately 10%,—between the grades in the middle of the room and the grades found in one or two rows at the rear. We shall appeal by way of illustration, to a lecture-group which bears out these facts in a distinctive manner. In Table I are put down the actual grades received by students in a course in ——— given during the first semester of 1916-17. Two preliminary quiz grades, the final examination grade, and the final grade for the course are included. The averages show clearly enough that the individuals occupying front seats have lower grades than those in the second and third rows, the difference being greatest between the first and fourth rows. In like manner, the individuals at the back of the room (row 8) are 18.7% lower than the average of the fourth row.

This striking difference varies during the course of the semester, as can be discerned in Table II, which is a further analysis of Table I. For example, the average grades at the time of the

TABLE II

Row	Av. 1st Quiz	Av. 2nd Quiz	Av. Final Exam.	Av. Sem. Grade
8	52.2	63.0	65.7	65.7
7	62.5	78.6	80.9	80.6
6	65.4	79.4	83.2	79.0
5	69.8	74.7	80.1	79.0
4	70.1	78.2	85.7	82.5
3	71.6	75.7	80.0	76.6
2	68.5	67.7	82.4	77.4
1	66.5	67.5	79.0	76.2

first written quiz decrease from the third row in the following manner: 71.6, 70.1, 69.8, 65.4, 62.5, 62.2, giving a maximal difference of 19.4% It will be observed that 10.3% of this difference falls between the seventh and eighth rows, so that the seventh row is 9.1% lower than the third row. As the semester proceeds, however, the individuals in the sixth and seventh rows improve with respect to the central group, with the result that, at the time of the final examination, the average from the sev-

enth row is but 4.9% lower than the highest average, which falls at the fourth row. It is obvious that some factor, partly overcome during the semester, serves to act as a handicap to the individuals at the back of the room. The large initial difference between the seventh and eighth rows and its persistence during the semester is characteristic of many of our tabulations, and the cause is to be sought, apparently, in some disadvantageous condition which especially hampers these students in the eighth row for the entire semester. Occasionally, however, the back row does recover and then no appreciable difference in distribution is observable in the final grades. That there is still a difference in the degree of accomplishment within the group at large, however, may be seen from the fact (cf. Table III) that ¼ more students

TABLE III

Row	Below 40	40-49	50-59	60-69	70-79	80-89	90-99	% Below 70	% Above 70
8	1	0	5	4	3	2	0	66	34
7	3	2	2	2	13	9	9	23	77
6	0	2	1	6	7	14	6	25	75
5	0	1	2	4	9	8	4	25	75
4	1	0	1	4	10	12	8	17	83
3	0	0	2	4	14	10	2	19	81
2	1	0	3	10	9	11	5	36	64
1	2	1	3	6	7	7	5	39	61

in the front than in the middle receive grades below 70%, while over twice as many in the back rows are graded below 70%. There is a tendency, therefore, more marked at first, for high grades to be grouped in the middle of the room. The significance of this distribution is made clearer by the fact that, at the front of the room, a little over a quarter of the students advance in their grades as the semester goes on, while, at the back of the room, more than one-half of the grades improve during the semester. In other words, the poor accomplishment of students at the front and at the back, at the time of the first written quiz, seems to act as an incentive to greater efforts and the improvement that is shown during the semester is not at all equaled by the improvement of students in the middle group, for whom there is, presumably, no such incentive.

The large difference between grades from the middle and from the front and rear sections of a room, at the time of the first

written quiz, with the somewhat smaller difference at the end of the course, is more significant than at first appears. It has been found, for example, that the difference is more pronounced in courses where relatively greater emphasis is placed on the material given in lectures. Students who are quizzed but once a week on two lectures show about twice as great a difference as appears where one lecture furnishes material for one or more quizzes and one or more laboratory hours. Furthermore, an informal lecture method (adopted most frequently with small groups) shows on the average a still smaller degree of difference, the variation in this case being but three or four per cent. That is to say, there is not here so large a difference between the number of students who receive exceptionally low or high grades in various parts of the room.

There are other peculiarities to be noticed. If a group of students is removed from the main body by an aisle or by a group of empty seats, their grades are on the average 5-10% lower than the grades of students sitting in the main section. For example, in Lecture Group C an aisle running parallel with the platform separated about fifty students from the main group of approximately 400. The final average of the students just in front of the aisle and belonging to the main group was 68.0. The average of the 50 students just behind the aisle was 61.0. In Lecture Group D the two aisles ran at right angles to the platform. The main body of students sat in the area directly in front of the lecture platform. Some 25 students sat across the aisle to the right and an equal number across the aisle to the left. The average performance of the central group was 80.3 and of the lateral groups, 76.0. In this later case the difference is materially increased if the grades of students at the rear of the rooms are omitted. Again, within the range of the rooms here included, which seat from 70 to 500 students, the difference does not seem,—as one might expect,—to be a function of the absolute size of the room. Again, the fact that practically all of our groups have shown the decrease in question indicates that the difference is not essentially dependent on the lecturer; although we have seen that an informal method with small groups

may result in a smaller difference. And, finally, an area of low grades at the rear of a small group in a large lecture room may coincide with the area of maximal grades for a large group in the same room. That is to say, the region of low grades so moves with the periphery of an audience as that low (peripheral grades of a small group actually fall in the very same seats as do the high (central) grades of a large group.[5] This fact can be seen to advantage in Figure I, where the curves represent the average grades of different rows in classes occupying seven, eight, and ten rows.

To summarize: The statistical treatment of student's grades suggests that there is an appreciable difference between the work and the accomplishment of individuals who occupy a central position in a lecture room and the work and accomplishment of those who occupy the outlying sections. This difference, which is greater at the time of the first quiz than at a later time, is also influenced by a natural division between groups, such as an aisle or a few empty seats or by pillars, and by the degree in which the course is dependent on lectures. Frequent small sectional and laboratory meetings tend to reduce the difference, which appars to be dependent upon the position of the student with reference to the rest of the group.

Now there are three factors directly related to the distance between auditor and speaker which might tend, in a large group, to bring about the differences of performance which we have found. In the first place, there is a difference in the perceptual

[5] In all the averages we have given, the *m. v.* is, as a rule, nearly if not quite as large as the differences we have cited as existing between the center and the periphery of a room. It must be remembered, however, that the one factor of differences in mental ability accounts for a large part of the *m. v.* When proper allowance for this factor is made by discovering the performance of the same or similar students in groups of thirty or less, the *m. v.* of the middle group is negligible; but at the front and especially at the rear the *m. v.* is still large and must be accounted for. We are inclined to urge that the large *m. v.* in these regions is not an indication of unreliability or of inadequate sampling but that it is further evidence that the individuals in these areas are working under some kind of a handicap. A few overcome the handicap and obtain average or exceptional grades, while the remainder succumb and receive exceptionally low grades.

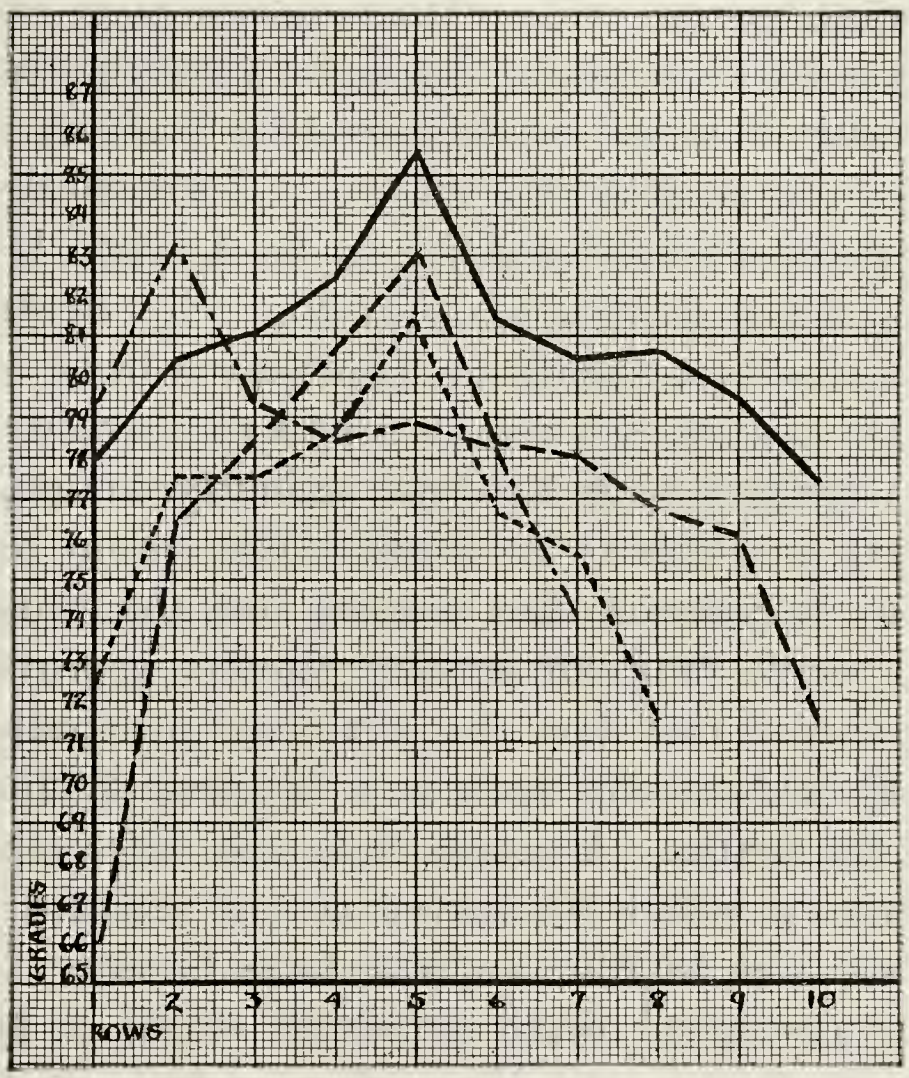


FIG. I.—Distribution of grades by rows in four lecture groups in different subjects but meeting in the same room.

factors dependent upon the distance of the student from the lecture-desk. It is apparent, of course, that while the acoustic properties of lecture-halls widely differ, in any one of them students sitting moderately near a lecturer have better opportunities for hearing. The same is true of visual factors, for frequently the success of a lecture depends largely upon demonstrations involving small objects and delicate manipulations, upon charts having inadequate size or illumination, and upon the facial expression and movements of the speaker. Moreover, lecturers differ in their ability to enunciate clearly as well as in the intensity and in the carrying power of their voices.

In the second place, there is a difference in the direction of attention resulting from the distance of the student from the lecture-desk. Our results suggest that there are three zones in which the direction of attention changes. We have found, for

example, that, on the whole, individuals sitting very near the lecturer have lower averages than those a little further removed. In these cases, it is not at all impossible that the attention of the student is taken up with a variety of irrelevant details. That is to say, there are facial changes in the speaker, idiosyncrasies of clothing and of gesture, and a host of other items that thrust themselves upon the attention of those near at hand. Unessential details of the apparatus are also a source of distraction. Further back, these details are lost and here, curiously enough, the highest averages are generally to be found. This group is in the best position for attention to the meaning and the sequence of the discourse. At the rear, still another situation appears. We have already indicated that here the perceptual processes may be at fault. This undoubtedly leads to a frequent shifting of attention from the lecture. It seems, then, that there may be an optimal distance at which the perceptual and attentive factors are the very best. In addition to these facts, lecturers differ in the amount of perceptual detail presented which is apt to draw attention. Lecture-rooms differ, also, in the distance between the lecture platform and the first row of seats. Furthermore, some lecture sections make a practice of leaving two or three vacant rows at the front. All of these factors would bear a direct relation to the steadiness and the direction of the attention elicited.

There is still another factor which may account for the discrepancy of performance between groups near to and remote from the lecturer. This factor is a result of the kind of instruction which students directly or indirectly give themselves and it is doubtless dependent, in large measure, upon the factors which we have already discussed. Among students sitting near a lecturer, this instruction may take either of two forms. The student may, in a large course, congratulate himself on having a fortunate location and so make it a means of getting all that goes on; or he may be subject to a negative instruction expressed in the words: "I am near the front. It will be so easy to get everything that I need not exert myself." As regards the group in the center, we find that it is again in an optimal position; these individuals

are not near enough to be cognizant of everything, relevant and irrelevant, that is going on; and neither are they so far away as to be in doubt. At the extreme rear, however, there are, again, two kinds of instruction. Some individuals, appreciating the fact that they are working under a handicap, realize that they must give special attention, if they are to maintain their standard of work. The statistical fact that there are about as many high grades at the rear as there are low grades suggests not only that perceptual and attentional factors need not be determinative; but that this kind of instruction plays a major part, especially after the first quiz, when a low grade adds an incentive to improvement. On the other hand, there is a group of individuals who take the negative instruction that since they have been placed where they cannot well see or hear they will give up the effort. Once again, the statistical fact that the low averages in the rear are due not so much to a general lowering of the grades as to a larger number of excessively low grades suggests that this instruction is too often taken.[6]

But when we have made due allowance for such physical factors as distance and intervening objects, factors which directly bear upon the adequacy of perception and the degree and steadiness of attention, there still remains a difference in performance to be accounted for. The fact that the low grades of a small group may exactly coincide in place with the high grades of a large group in the same lecture-room suggests that there is a factor directly dependent upon the group itself. Now it is a commonplace observation that individuals in the periphery of a large crowd are apt to be restless and inattentive to whatever may be attracting the interest of the main group. That is to say, physical compactness and the interests and activities of a group polarized toward the speaker tend to knit together the

[6] For example, note the following quotations taken directly from students. When asked what the trouble was, one replied: "Well, I'll tell you. It was just like this. In all my other courses I sit near the rear and I have to pay attention in order to know what's going on. But in this course I was right up at the front, and it seemed a cinch so I didn't care much what notes I took." Or, again, "Well, one thing, I think, is this. They put me away back in the rear. I couldn't see or hear very well and I just lost interest."

main body of an audience in a way that is not possible for individuals seated near the borders of the group. That this factor of social integration plays a large part is indicated by the effect of aisles or other marks of separation, by the effect of a dialectic or informal mode of address, by the decrease in the differences between the optimal region and the peripheral regions as the semester goes on and as social integration becomes presumably greater, and finally, by the fact that relatively low grades always come from the periphery of a group, no matter how small or how large,—within the limits of the audiences here investigated,—the lecture group may be.

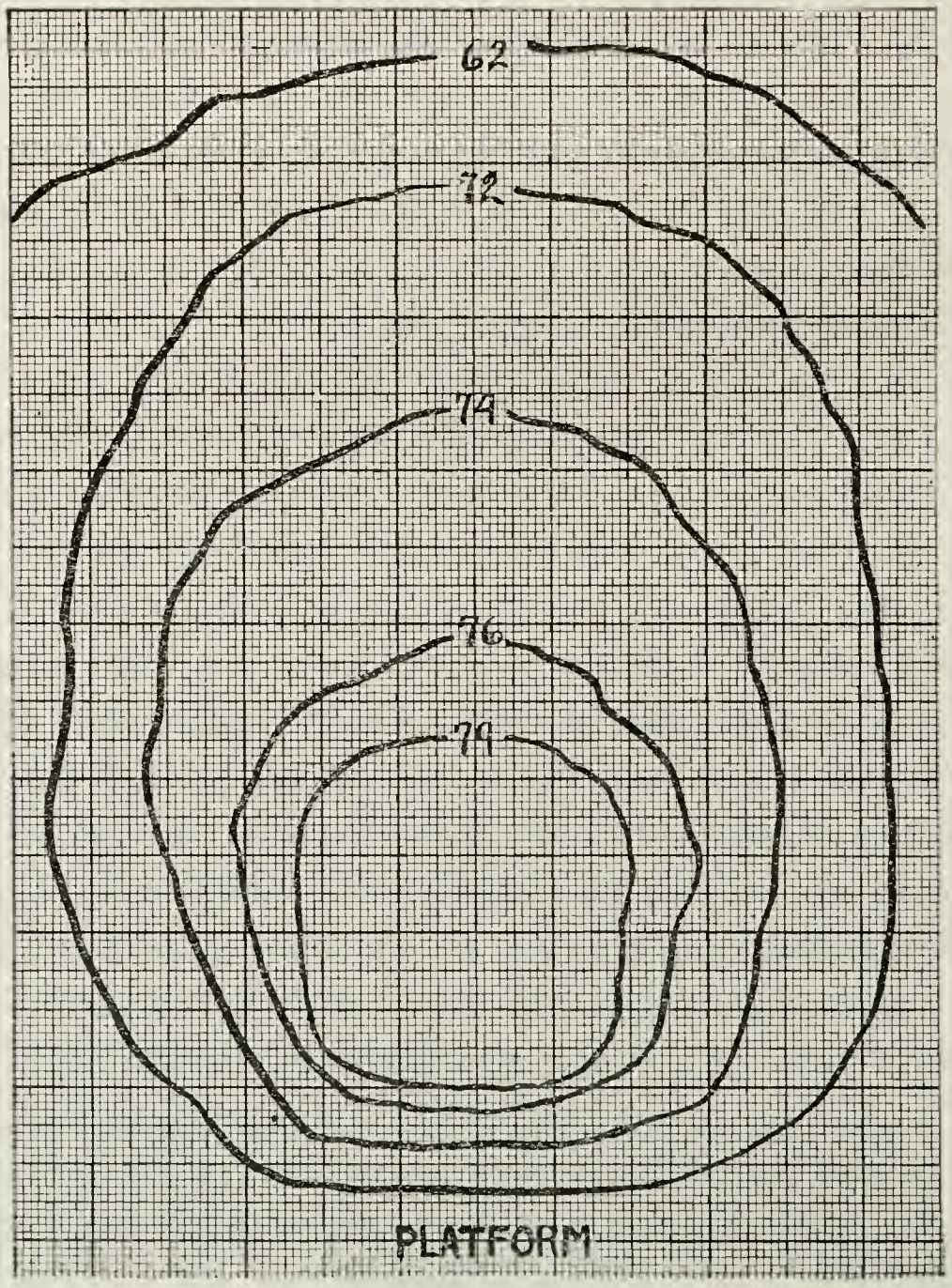


FIG. II—Topographic chart representing the approximate lines of performance of an audience early in the semester. Aisles or other obstructions would materially affect the "social gradients."

We are justified, it seems, in speaking of the topography of the audience where heights and depths are measured by degrees of social integration and hence amounts of individual achievement. If our tabulations represent the facts, a typical audience could be represented as in Figure II, where the lines represent levels of performance or degrees of achievement (expressed in numerical grades) and thus degrees of social integration. That is to say, a lecture audience is a polarized audience with reference to the speaker; but it is also integrated with respect to itself.

Our investigation has shown, in fine, that there is a well-marked variation in academic grades which is not due to a difference in mental ability or to other factors referable to the individual auditors. Neither is the variation due entirely to the distance of the auditors from the speaker. On the other hand, it does seem to be directly dependent upon position with reference to the total group. That is to say, when due allowance has been made for the factors mentioned above, and for the resulting mental organizations, a residual variation seems essentially to rest upon the varying degrees of social integration among the members of the group.

LEADING AND LEGIBILITY

By Madison Bentley

In the literate adult, reading is a rapid, smooth-running, and highly automatized performance, which may go on for a long time without noteworthy lapse or hesitation. It appears to be a simple operation, but it is not. Even in highly practiced subjects it can scarcely be called "simple," because it depends upon the coöperation of a large number of heterogeneous conditions. In the first place, the constant readjustment of the eyes to the page is complicated. The leaps and pauses which the eyes make in reading involve a complex and delicate mechanism. Again, the total state of the central organ and its functional tendencies, taken together with the mental concomitants of attention, associability, and the touching off of meaning, form a second set of conditions upon which reading depends. And, finally, the apprehension of the written or printed characters rests upon a large number of local factors which may retard or facilitate the process. In the reading of print, for example, the color and lighting of the page and of the print, the size and form of the type, the length of the line and the spacing of the letters, all have their effect upon the rapidity and the ease of reading.

These three sets of conditions, which we may roughly distinguish as peripheral, central and mental, and typographical, have suggested to psychology a great many problems. The conditions are all closely interrelated; but experimental progress has been made only when one factor or another has been isolated for study.[1] In our present small investigation, we have tried to isolate one of the "typographical" conditions;[2] the factor of

[1] The psychology of reading has already acquired a large bibliography. Most of the older titles may be found in Huey, E. B., *The psychology and pedagogy of reading,* etc., New York, 1912. A number of recent pedagogical studies of reading are exemplified by the monograph, Gray, C. T., *Types of reading ability,* Chicago, 1917.

[2] A partial list of "typographical" conditions, naming twelve factors of this sort, is given by Legros, L. A., and Grant, J. C., in their large work on *Typographical printing surfaces,* etc., London, 1916, pp. 156-157.

"leading" or—more accurately—of the vertical distance between printed lines. The "leaded page" is a page in which the type-setter has inserted one or more thin metal strips (the thinnest, 1-point, lead is about 1/72 inch thick), called "leads," between the successive lines.[3] We do not here directly consider the type-written page, where only two spaces between lines, single-space and double-space, are commonly used.

In Roethlein's study[4] of the relation between legibility and the characteristics of the type-face, it appeared that the rate and the ease of reading were affected—among other factors—by the amount and disposition of the blank space around each letter. This fact, taken together with the obvious difficulty of reading a crowded and close-set page, suggests that the open space between succeeding lines may be an important condition of legibility. The study, then, of this condition is our present problem.

Materials and procedure. The type-face used was of the style "monotype,"[5] a close approximation to "news gothic," a style which Roethlein found to stand relatively high in legibility. Figure I reproduces samples of our reading texts set in 12-point face and body, without leads (a) and with 7-point leading (b).

At first we used a large type (thicker and cleaner than the reproduction in Fig. I) in order that the sheets might be presented through a wide range of distances, and so offer to the reader varying degrees of difficulty. The type (12-point) was impressed on fine heavy paper, exactly ten full lines on each sheet. The line was 3 5/16 inches long, and the sheet was 6⅝

[3] The printers' unit of linear size is the "point" = 0.013837 in., approx. 1/72 in. Twelve points make the "pica," approx. 1/6 in. The interlinear space is not necessarily equivalent to the leading, since the type-face is sometimes smaller than the type-body. Thus a 10-point face upon a 13-point body would give the appearance upon the page of 3-point leading. We shall use "interlinear space" as the distance between the extreme projections (ascenders and descenders) of the small letters in succeeding lines. In our case, it will be the equivalent of the points of leading.

[4] Roethlein, B. E., The relative legibility of different faces of printing types, *Amer. J. of Psychol.*, 1912, xxiii, 1-36, esp. p. 28.

[5] The printing was done with great care and pains by Messrs. R. R. Donnelley & Sons Company, Chicago, to whom the Laboratory stands under a heavy debt. It was arranged for and followed through the press by Dr. Carl Rahn.

I once knew an unworldly-minded man down in Maryland who built himself a house. The desire of his heart was to have a spacious entrance hall, one that would sound a key-note of hospitality and give an air of spaciousness and taste. When he got to planning the house, he found that he would have to build a large living-room to justify this much-desired hall. He had never a thought of a library until he saw that one was needed

FIG. Ia

According to a well authenticated report, they have no serious objections to entering the republican caucus if they are invited properly and joining in naming a candidate for speaker and otherwise participating in the framing of the republican legislative policy. The full political significance of such possible action carries with it the idea that, for the first time since the 1912 smash-up, there would be a real working agreement between

FIG. Ib

x 7½ inches. The sheets were set upon an exposure-frame which was movable along an optical bench in the dark-room. The whole was surrounded by a rectangular wire frame covered the black cloth, making a long rectangular tunnel, 9 in x 12 in. The subject was adjusted to a head-rest and looked into the black tunnel down which he could see the printed sheet. The printing was evenly illuminated by concealed electric lamps and by reflectors. After a warning signal, the field was suddenly exposed by the experimenter, and the subject read aloud the ten

lines at top speed. An indicator showed the subject in advance just where the first line would begain, and he recorded his beginning and ending times by means of a finger key and an electrically controlled stop-watch.

In order to ascertain the visual angle ordinarily subtended by the length of the line as printed in books, magazines, and newspapers, and also to procure variety in the substance of the excerpts to be read by our subjects, we took extracts from eight books, written upon a variety of topics, eight magazines, serious and popular, and eight newspapers, representative of this country and England. We gathered in this way materials for 90 extracts, each 10 lines long. Thus we had 9 excerpts for each of 10 leadings (0 and 1, 2, 3 . . . . 9-point), and the whole 90 were exposed in a mixed order to each of our readers. We took care that the order observed should equally distribute the variable error of practice to all the 10 degrees of leadings used at a given reading-distance. The effects of practice which may have been cumulative as the experiments passed from one reading-distance to another are not wholly eliminated because we did not at first anticipate the exceedingly wide range of distances which we were to use. These effects were practically cancelled by carrying 2-4 distances on together. They have no special significance for our problems. Fatigue was avoided by short periods of reading and frequent intervals of rest. We kept, so far as we could, to the same hour in the day for a given subject; although our whole series of experiments, running through four years, covered nearly the entire day from 8 a. m. to 5 p. m.

Before the reading began for the hour, the following instructions were read to the subject:

"At a signal 'ready' you will be shown a grey exposure field. Fixate the white pointer. At a second signal, 'now,' the grey field will be removed, disclosing a block of printed matter. Begin immediately to read aloud and at the same time press the key at your right hand. Read as clearly and as rapidly as you can. Do not correct errors. If a strange or an unclear word appears, say 'blank' and continue reading. As you read the last word press the key a second time. After the reading, report the number of mistakes made."

We at first proposed to include an introspective study of the processes involved in reading and especially those affecting the rate; but we later discovered that such a study properly belonged to a separate inquiry.[6]

According to a well authenticated report, they have no serious objections to entering the republican caucus if they are invited properly and joining in naming a candidate for speaker and otherwise participating in the framing of the republican legislative policy. The full political significance of such possible action carries with it the idea that, for the first time since the 1912 smash-up, there would be a real working agreement between

FIG. IIa

According to a well authenticated report, they have no serious objections to entering the republican caucus if they are invited properly and joining in naming a candidate for speaker and otherwise participating in the framing of the republican legislative policy. The full political significance of such possible action carries with it the idea that, for the first time since the 1912 smash-up, there would be a real working agreement between

FIG. IIb

We drew our readers, 18 in all, from the departmental staff, from graduate students of psychology and from advanced under-

[6] In the first four thousand and more readings, the experimenter carefully followed the reading, with a duplicate copy of the excerpt before him, and set down on a prepared blank the errors committed. The errors consisted of hesitations, blanks, wrong words, new insertions, attempts at corrections, breaks in the voice, nervous laughter and ejaculations. We had hoped to relate these lapses, as well as the mental processes which underlay them, to the time of reading the ten lines; but that also proved to be impracticable because of the variable factors of sense, meaning, and degree of familiarity. The introspective study still lies before us to be completed.

graduates, most of whom were pursuing experimental problems.[7] It was soon found that the subjects best adapted to the problem were those who maintained a steady and constant reading "set" which kept them under pressure to do their best and to complete the reading in the shortest possible time. In a comparative study of reading-times much depends upon the steadiness of this psychophysical determination. All the readers save R read without knowledge of the problem. The substance of the excerpts had, of course, its influence upon the rate of reading; but our wide choice (90 extracts from a wide variety of topics, ranging from epistemology to light fiction and the fashions) was designed to distribute to the various leadings the influence of the meaning-substance upon rate. The results seem to justify our assumption.[8] The whole number of readings was 6640. As the experiment progressed, it seemed advisable to secure a wider range of the size of type and also of reading distances. To this end, we procured from the University's photographer photographic reductions of our sheets set in 12-point type. These reductions were made on the scales of 1.0 to 0.72 and 1.0 to 0.60, and they represent approximately 9-point and 6-point types. In the re-

[7] The experiments are the work of many hands. A large part of the earlier work of planning, devising, and overseeing fell to Dr. Carl Rahn, who also served as observer. One whole division fell under the guidance of Professor C. A. Ruckmick. The experiments were placed in the competent hands of Mr. W. T. Doe, B.A., and Mr. L. C. Raines, B.A. Some of the later series were carried through by Miss M. Jones. The readers who were good enough to give their time and their enthusiastic effort were Broom (Br), Bentley, Dr. R. C. (B), Cuthbert (C), Carman (Ca), Carlsen (Cl), Fluke (F), Fera (Fe), Griffith (G), Goebel (Gb), Gould (Go), Greene (Gn), Gross (Gr), Knapheide (Kn), Kohl (K), McKinney (Mc), Rahn (R), Raines (Ra), and Rutherford (Ru). The graphs have been drawn by Dr. C. A. Griffith.

[8] The Laboratory is to carry through another study which makes use of nonsense syllables and nonsense words. That sort of reading will, of course, be very different (in character and mechanism) from the perusal of the ordinary sense page.

[9] An independent variation of type-size and leading would have been preferable to our photographic reductions, but the expense of the typesetting was formidable. We shall speak of 0-lead, of 7-leads, etc. in the photographic reductions, for the sake of simplicity; although the actual interlinear distances have been changed.

ductions, the thickness of the single lead (1/72 inch) became approximately 1/100 inch and 1/120 inch.[9] We might have kept to our large type and regarded only retinal size, but we did not care to assume that a large type at a great distance would be read with the same facility as a small type at a smaller distance, *provided only* the visual angles subtended by the two types (and, of course, the two line-lengths) were the same. The conditions of reading are too complex to warrant such an assumption. Habitual reading, *e.g.*, at a given distance (as the "normal reading-distance" for the emmotropic eye) might favor that distance from the eyes. So we used the three sizes of text at many distances. In general, the 6-point and the 9-point were

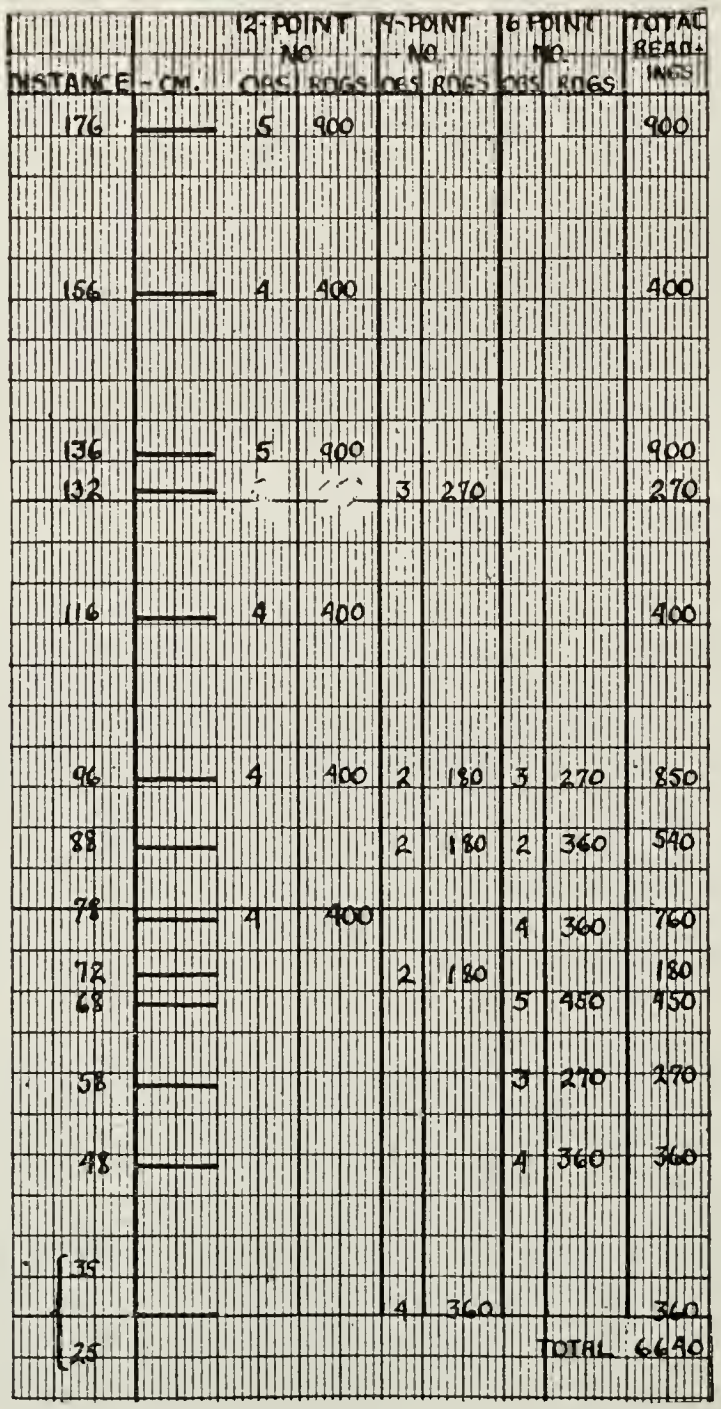


FIG. III

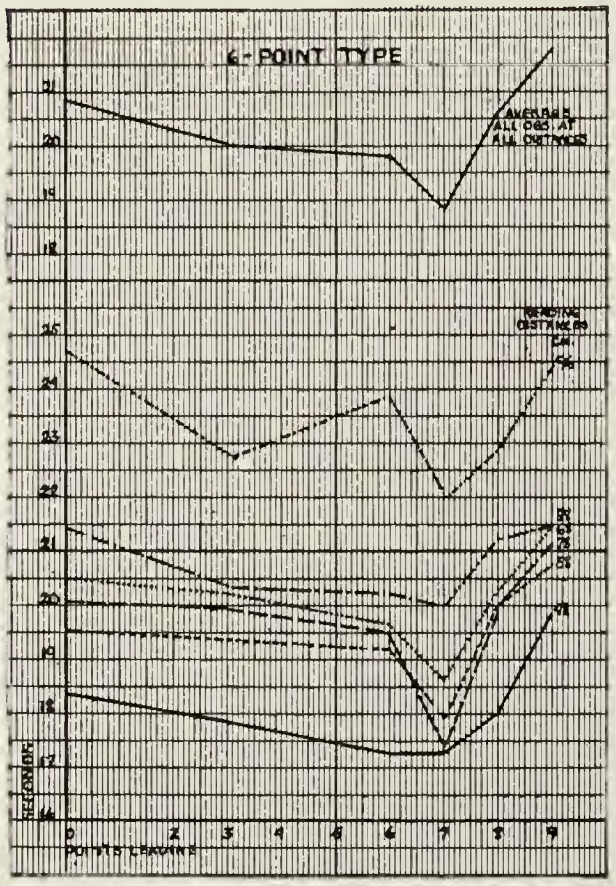
read at the smaller distances. Figure II shows samples (7-point leading) of these two blocks of type, while Figure III displays the distances at which each type (at the various leadings) was read.

The readings taken at a distance of 25-35 cm. call for a special description. It seemed advisable to compare our results acquired in the dark room under somewhat unusual conditions with printing which should be presented to the subject as he ordinarily reads. To this end we arranged a reading desk at an optimal inclination and with good indirect light derived from a large window obliquely behind the subject. We used the same care as elsewhere in the exposure. The reader made his own finer adjustment to distance by moving his .head backward and forward while a preliminary card rested upon the reading desk. The distance chosen was read off (cornea to plane of printed sheet) just before beginning and just after finishing a set of excerpts. With our 9-point types, all the distances fall within the limits of 25-35 cm. (ca. 10—14 in.) The range was fairly narrow for any single reader.

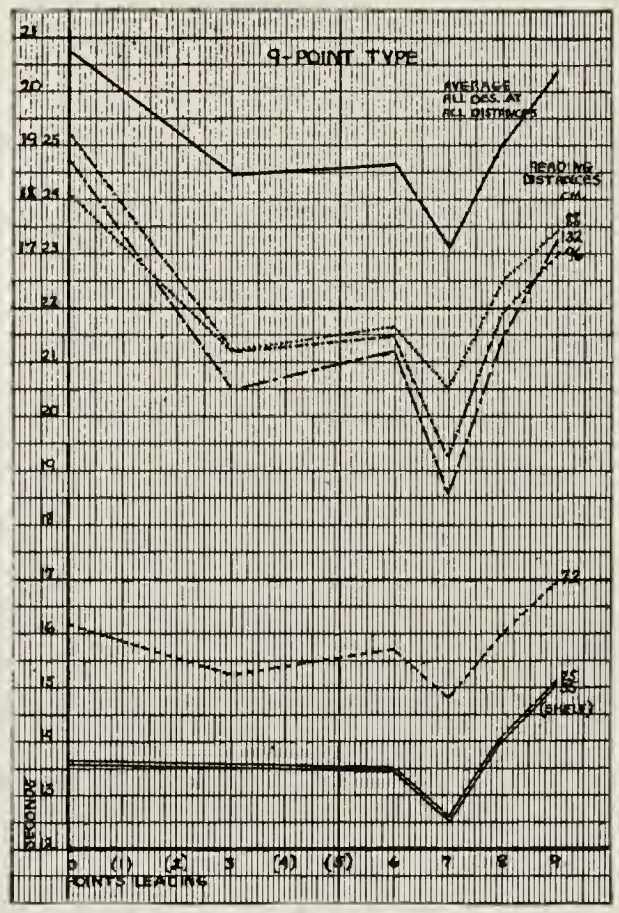
Results: The general trend of the reading-times at the various distances and with the ten leadings, 0-9, is shown upon our graphs. Here the number of leads (separation of the lines) is set down at the bottom of the sheet and the time (sec.) expended on reading the ten lines of printed matter is set upon the line of ordinates. After a careful inspection of results we decided to include in the graphs only leads 0, 3, 6, 7, 8, 9. The others appeared to have no special significance. All the times for any one size of type (6-, 9-, or 12-point) are gathered together in one place, each graph standing for all readings at a given distance. Above each group is a compound graph which combines all those beneath it, *i.e.*, all 12-pt., or 9-pt., or 6-pt. results. The unbroken lines in each group represent one and the same set of observers, the broken lines another set, and so on. The times of the individual subjects (ave. of each reader's trials at a given distance) are given in figures in Table I.

As regards the reading-times for different distances and for different type-sizes, the inspection of the graphs will make it

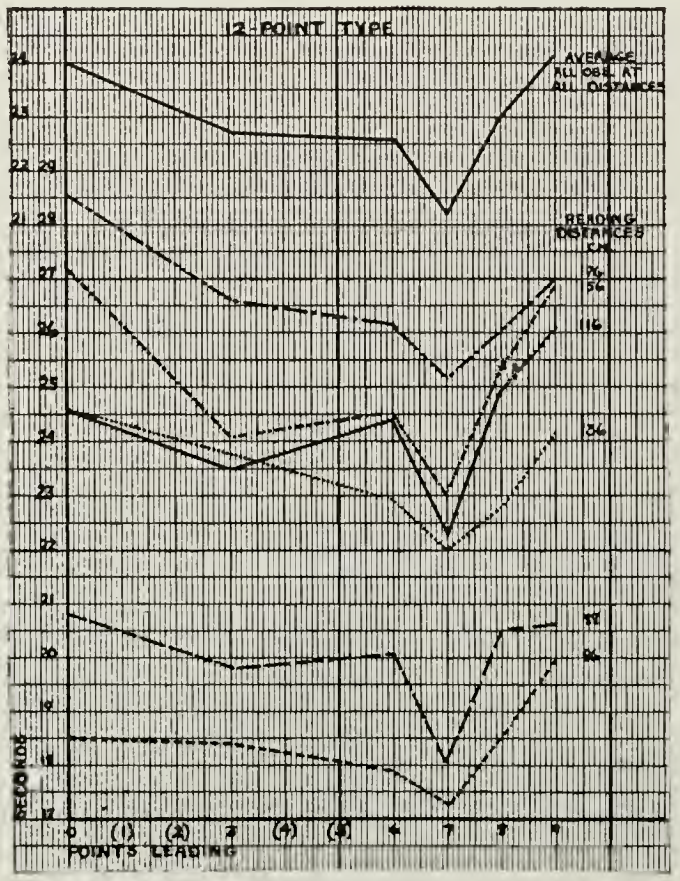
apparent that, in general, the greater the distance of the page from the eye the longer the reading-time. This rule obtains where the group of observers is the same for different graphs and also,—usually, not always,—where different observers read at



GRAPH I



GRAPH II



GRAPH III

TABLE I

12-Point Type

Subj.	0	3	6	7	8	9	Lowest
Distance = 176 cm.							
B	23.4	22.9	22.5	20.8	21.3	22.8	7
C	26.8	26.5	26.4	25.2	26.6	28.3	7
F	30.6	30.2	29.3	29.1	28.9	30.7	5(28.5)
G	18.0	17.0	16.1	16.0	17.1	17.3	4(15.9)
R	43.1	36.6	36.9	34.8	36.1	35.9	7
Ave.	28.4	26.6	26.2	25.2	26.0	27.0	
Distance = 136 cm.							
B	19.4	19.2	18.2	17.9	18.2	19.5	7
C	29.8	28.2	28.5	26.7	27.9	30.1	7
F	23.0	23.2	23.5	22.3	23.1	25.0	7
G	17.4	16.4	16.3	16.6	16.7	17.8	4(15.9)
R	33.2	31.1	27.4	26.7	25.4	28.5	8(25.4)
Ave.	24.6	23.6	22.8	22.0	22.7	24.2	
Distance = 156 cm.							
Ca	26.6	25.1	25.6	23.7	25.5	29.5	7
Gr	31.1	25.0	27.3	25.6	28.4	30.1	3
Mc	31.5	28.5	27.6	26.1	30.0	29.9	7
Ra	19.7	17.8	17.6	16.5	17.4	18.2	7
Ave.	27.2	24.1	24.5	23.0	25.3	26.9	
Distance = 116 cm.							
Ca	21.1	22.3	23.1	21.3	24.2	26.9	1(21.1)
Gr	25.6	23.2	24.8	22.3	24.5	25.2	7
Mc	26.6	23.7	24.0	22.8	24.4	26.2	7
Ra	25.2	24.9	25.6	22.6	26.5	26.2	7
Ave.	24.6	23.5	24.4	22.2	24.9	26.1	
Distance = 88 cm.							
Ca	24.7	24.2	24.2	21.8	24.9	24.5	7
Gr	18.7	17.3	18.7	17.2	18.5	18.5	7
Mc	23.6	22.4	21.7	19.3	22.4	23.3	7
Ra	15.9	15.3	16.0	14.1	16.3	16.1	7
Ave.	20.7	19.8	20.1	18.1	20.5	20.6	
Distance = 96 cm.							
Cl	22.8	22.1	21.5	20.1	20.7	22.8	7
Go	18.2	18.7	17.2	18.3	18.9	20.5	6
K	17.5	17.1	17.3	15.6	17.5	18.7	7
Ra	15.4	15.6	15.1	15.1	17.1	17.6	6 & 7
Ave.	18.5	18.4	17.8	17.3	18.5	19.9	

9-Point Type

Subj.	0	3	6	7	8	9	Lowest
Distance = 132 cm.							
Fe	22.8	19.1	18.8	16.0	17.5	21.3	7
Gn	28.4	21.2	24.0	20.6	25.2	26.4	7
Ru	24.0	21.4	20.9	19.3	21.4	22.0	7
Ave.	24.7	20.6	21.2	18.6	21.4	23.2	
Distance = 72 cm.							
Fe	14.4	13.6	13.8	12.7	14.1	15.2	7
Ru	18.0	17.0	17.7	16.9	17.9	18.8	7
Ave.	16.2	15.3	15.7	14.8	16.0	17.0	
Distance = 96 cm.							
Cl	30.0	23.4	23.8	20.6	24.0	24.4	7
Gn	20.4	19.0	19.2	18.0	19.6	21.8	7
Ave.	25.2	21.2	21.5	19.3	21.8	23.1	
Distance = 88 cm.							
Cl	26.7	22.4	22.1	21.6	24.3	25.0	2(20.3)
Gn	21.5	20.0	21.2	19.4	20.5	21.8	7
Ave.	24.1	21.2	21.6	20.5	22.4	23.4	
Distance = 25-35 cm.							
Br	11.9	12.1	11.9	10.7	11.9	13.0	7
Cl	11.8	11.4	11.4	11.0	11.7	14.0	7
Kn	15.8	15.6	15.4	14.6	16.2	17.1	7
Ru	15.3	15.5	15.5	14.6	16.2	16.9	7
Ave.	13.7	13.6	13.5	12.7	14.0	15.2	

TABLE I (Continued)

6-Point Type

Subj.	0	3	6	7	8	9	Lowest
Distance = 96 cm.							
Gr.	27.1	24.7	25.3	23.4	24.4	26.4	5(22.4)
Mc	30.7	27.8	29.8	27.4	27.5	29.5	7
Ra	16.4	15.9	16.4	15.6	16.5	17.2	7
Ave.	24.7	22.8	23.8	22.1	22.8	24.4	
Distance = 68 cm.							
Cl	28.8	25.5	23.9	22.7	25.1	24.8	7
Gb	18.6	17.1	17.6	15.3	17.0	19.0	7
Go	21.1	20.1	20.3	19.2	21.5	22.5	7
K	17.0	20.7	19.6	19.5	20.4	21.3	1(17.1)
Ra	16.8	18.1	16.6	16.2	17.6	19.4	7
Ave.	20.5	20.3	19.6	18.6	20.3	21.4	
Distance = 88 cm.							
C	27.0	25.9	26.3	26.0	27.4	26.7	2 & 5
G	15.8	15.0	14.3	14.1	15.2	16.2	7
Ave.	21.4	20.4	20.3	20.0	21.3	21.4	

Subj.	0	3	6	7	8	9	Lowest
Distance = 58 cm.							
Gr	19.0	17.6	17.2	17.2	17.8	19.0	5(16.7)
Mc	24.0	24.1	24.0	21.2	24.4	25.3	7
Ra	15.9	16.5	16.4	15.2	17.8	18.2	7
Ave.	19.6	19.4	19.2	17.9	20.0	20.8	
Distance = 48 cm.							
Cl	22.3	20.7	20.1	20.5	20.5	23.2	4(18.8)
Gb							
Go	17.2	17.5	17.6	17.3	18.3	19.5	0&2(17.2)
K	17.3	16.3	15.8	15.8	15.8	18.2	1, 2, 6, 7&8
Ra	16.6	16.8	15.9	15.7	17.3	17.8	7
Ave.	18.3	17.8	17.3	17.3	18.0	19.7	
Distance = 78 cm.							
Ca	21.1	19.4	20.2	18.0	21.0	21.6	7
Gr	17.5	20.6	19.9	15.9	18.1	20.0	7
Mc	26.1	24.2	22.6	21.0	24.2	25.4	7
Ra	15.6	15.4	15.5	14.8	17.0	17.3	7
Ave.	20.1	19.9	19.5	17.4	20.1	21.1	

different distances. Reading of 9-point type at the common distance of 25-35 cm. (in daylight), 12-point type at our shortest distances, 88 cm. and 96 cm., and 6-point type at 48 cm. give the shortest general times.

The most obvious regularity in the numerical result appears upon a comparison of the three "Average" graphs at the top. In absolute time they differ somewhat, the curve for 12-pt. running highest (beginning at 24 sec. and ending at 24.1 sec.); but in form they are virtually coïncident. There is a steady decline in reading time from the type "set solid" (0-leading) to 3-point leads, little or no decline to 6-point, then a dip to 7 and a rise to 9. The two end-times, 0-lead and 9-point, are about the same. With all three type-sizes and at all distances the minimal time uniformly appears with 7-point lead, a result which is reproduced in most of the average times of all the individual subjects (see "Lowest" column in Table I).[10]

[10] This uniformity in the results appears especially noteworthy when we consider (1) that the range of average times for a Subj. throughout the

The amount of time actually saved in reading by the increase of the interlinear space from 0 to 7-point leads varies with the size of type and with the reading distance. Thus the optimal leading saves at the rate of about 8.5 minutes in the hour with 12-point read at 156 cm., and about 5 minutes in the hour at 88 cm. With 6-point, the saving is as much as 8 minutes (78 cm.) and as little as 3 minutes (48 cm.). The 9-point texts, which approximate much of the common type of our books and periodicals, showed a saving of the optimal over the minimal leading of about 15 minutes in the hour at a reading-distance of 132 cm.. At the usual reading distance (about 12 inches), however, the saved time fell to about 1/13, or a little more than 4 minutes in the hour. All of these reductions in time, it should be noted, are obtained with an exceedingly clean and legible type. The saving might be still greater with a bad type-face which should offer internal difficulties within the line.

The dip in the curves at 7-point leading is curiously constant and strikingly regular. It appears in the several graphs representing the several distances (25-176 cm.) and with all groups of observers. If the ends of the graphs are covered, exposing only the sections 6 to 8, it will appear that the dip or "V" is nearly symmetrical,—that the drop from 6 to 7 and the rise from 7 to 8 have practically the same inclination, *i.e.*, leadings 6 and 8 are read with approximately the same facility, while leading 7 is distinctly shorter in time. At this point—7 leads—the vertical distance between lines is, for 12-point type, 1/72 x 7 = ca. 1/10 in. For the (reduced) 9-point type, it is 1/100 x 7 = ca. 1/14

different leadings did not often exceed 5 sec., and (2) that the m.v. of the reading-times for any singe leading turned out to be relatively large by reason of the variety of topics treated in the ninety excerpts. The following is a typical instance.

	Leads	0	1	2	3	4	5	6	7	8	9
Subj. C at 136 cm.	Times (in sec)	29.8	29.4	29.4	28.2	27.8	27.2	28.5	26.7	27.9	30.1
	m. v.	3.9	2.0	3.2	1.3	3.1	2.8	3.2	3.2	2.3	1.6

The wide variation in reading-times arising from the greater or lesser difficulty offered by the text led us to depend less upon the m. v. as a measure of reliability than upon an accumulation of readings sufficiently extensive to eliminate the variable error due to the assimilation of meaning.

in.; and for the (reduced) 6-point, 1/120 x 7 = ca. 1/17 in. The dip, then, at a 7-point lead appears (a) at any given reading distance and with three different interlinear gaps: it also appears (b) throughout a wide range of visual angles. It seems, then, that the critical or optimal leading is not entirely a matter of the interlinear spacing, of the clear space left above and below the line, and not entirely a matter of the drop of the eye in passing from the end of one line to the beginning of the next. It appears rather to depend upon the *ratio*[11] of letter-height (H) to interlinear space (S); *i.e.,* upon the ratio H/S.

Now what the factors are which make our particular ratio for 7 leads (H/S = 1.7)[12] optimal for reading under our conditions, we can only conjecture. The determination of them belongs to another study. It is probable that the eye-movement is not alone concerned,[13] for our three line-lengths, $3^{5}/_{16}$, $2^{8}/_{16}$, and 2 inches, yield the same results; and if it were merely a matter of clear space about the centres of fixation upon the line, there is no reason why lines separated by 8-point and 9-point leads should be more slowly read than 7-leaded lines or why small type (as 6-point) should require only a small clear interlinear space while larger type requires a larger clear setting.

It is altogether likely that the conditions underlying this dependence of facility of reading upon leading are complex. We began with the observation that reading is a complicated operation. It appears now that a slight variation of a factor so simple as the interlinear space may involve complicated ocular movements throughout and between the lines, indirect vision, atten-

[11] It should be noted that our line-length is always a function of this ratio. The line-length follows letter-height wherever the visual angle changes, whether from shift of reading-distance or from photographic reduction of the text. The dependence of reading-rate upon the length of line we are pursuing further.

[12] The vertical height of the 12-point letter (including ascenders and descenders) is approximately 1/6 in. Seven-point lead, at 1/72 in. for each point = 7/72 in., and 1/6 ÷ 7/72 = 1.7.

[13] For a bibliography of the study of eye-movements see Schmidt, W. A., *An experimental study in the psychology of reading,* Chicago (Univ. of Chicago Press), 1917.

tion under distractors, and little known central conditions which are responsible for the emergence of meaning from the apprehension of the symbolic characters of the printed page. The methodical and technical means are at hand for the analysis of such a tangled set of conditions; but the application of them will require further experimentation.

Summary. It appears upon the evidence of the experiments that facility and rate of reading the printed page are dependent, among other factors, upon the vertical space left between the lines (leading). Under our conditions, unleaded, or closely set, matter was read with relative slowness. With increase of the interlinear space, the rate of reading increased up to 1/10 inch (7-point leading), and then rapidly declined.

THE PRINTING OF BACKBONE TITLES ON THIN BOOKS AND MAGAZINES

By P. N. Gould, L. C. Raines, and C. A. Ruckmick

To those who have been working in libraries containing magazines in their original covers and bound books of few pages it has probably become noticeable that a very unfortunate lack of uniformity exists in the printing of titles lengthwise on the backs of these magazines and books. While there is in different countries a general disposition to conform to some standard of printing, there is by no means an agreement on the subject in any one country. In general and with many exceptions, the European plan is to print such titles from bottom to top, while the American plan, if there is one, is to print in the reverse direction. A considerable amount of discussion has arisen on the subject, especially in connection with library practice and in the printers' journals.[1] There are related questions, such as the subscriptions to full-page plates inserted in reading material,[2] and the insertion of omitted matter in writing when the insertion is titled in one direction or the other.

Many persons, in discussing the subject, seem to take the view that while they may have individual preferences in the matter they would be willing to set these aside if a universal practice could be established. Libraries insist, for the most part, on the advantages derived from printing titles in the direction from top to bottom, arguing that when magazines are stacked on tables, newsstands, and temporarily on shelves, the logical direction, with the face up, would be from top to bottom, or, when they are in this position, from left to right.

Some preliminary investigations, though crude, have been

[1] *American Printer,* August 5, 1918. *Printing Art,* 1918, xxxii, 62. *Publishers' Weekly,* 1918, xciv, 1763, 1994.

[2] *Preliminary Report of the Joint Committee on Standards for Graphic Presentation,* Amer. Soc. of Mech. Eng., N. Y.

made on the question, but so far apparently no experimental work has been published. One preliminary study resulted in the decision of seven in favor of printing from bottom up and two in placing the lettering from top down. Three additional answers, however, were noncommittal. In another inquiry an investigator found that among men of his own staff who were preserving pamphlets and frequently writing titles themselves on the backbone, nine out of ten were using the method which involved printing from bottom to top. Three out of four of them declared that any other method was unnatural. On the other hand, so eminent an authority as Melvil Dewey says, "The statement that it has been demonstrated by tests to be easier and more natural to read from bottom to top contradicts all the experiments and investigations I have made for forty years."

Obviously, on further consideration, there are a number of circumstances which are indirectly involved in the question. If ease of reading is to be a criterion, then the way in which the head is more readily inclined undoubtedly has an important bearing. A great number of individuals, from replies received on the subject, indicate that it is easier for them to tilt the head to the left. And, indeed, there is some basis on the side of the musculature of the eyes, neck and shoulders which might favor this inclination.[3] It is possible, also, that the tilt of the head in writing, being for the most part to the left, has a bearing on the problem. One correspondent, for instance, noted that during the time that he was learning to read and write Hebrew and for some time thereafter his head would habitually tilt to the right instead of to the left. Another authority widely known for his investigations of eye-movement states that he has noticed the invariable practice of right-handed people tilting their penmanship upward, while left-handed individuals habitually write downward.

A relatively large number of individuals have remarked that a differentiation exists between the use of either hand in taking books from the library shelves, the left hand being used to

[3] See the discussion of head-movements relative to Listing's Law in Helmholtz, H., *Handbuch der physiologischen Optik,* 3rd ed., vol. iii, Hamburg and Leipzig, 1910, pp. 120-123.

withdraw the book from the shelf while the right is preparing to finger the pages, and in this position the body would be more inclined toward the left. All of these suppositions are conditioned by the height of the shelf above or below the level of the eyes, as well as by the location of the shelf to the right or to the left of the individual. Several replies indicate also that the lighting effect may be a complicating factor. Those who argue in favor of printing from top to bottom seem to be impressed by the analogy of reading printed lines from the top of the page down to the bottom. But it is not unlikely that the analogy is overdrawn and that the factors involved in reading titles of books, when printed at right angles to the usual movement of the eyes, presents a totally different situation from the ocular excursions of ordinary reading.

Experimental Series

Apparatus. The entire problem was taken over as an experimental investigation in the psychology laboratory, and conditions were devised to simulate those of the library. In one of the dark rooms of the laboratory a shelf was designed to be fastened in a number of positions on the wall, being adjusted to heights of four, five and six feet from the floor, these being standard heights in library practice. On the shelf about a score of books were placed and among them a so-called "dummy" was inserted. This dummy measured 26 cm. long by 2 cm. thick. Along its front edge a sliding arrangement was provided into which printed titles could be placed. These titles were printed on heavy white paper and consisted of thirty-eight different names of standard American publications such as:

THE GEOGRAPHICAL WORLD
THE AMERICAN FORESTRY
THE AMERICAN WOOL REVIEW
DUNN'S INTERNATIONAL REVIEW
THE MARINE REVIEW
THE CONTEMPORARY REVIEW

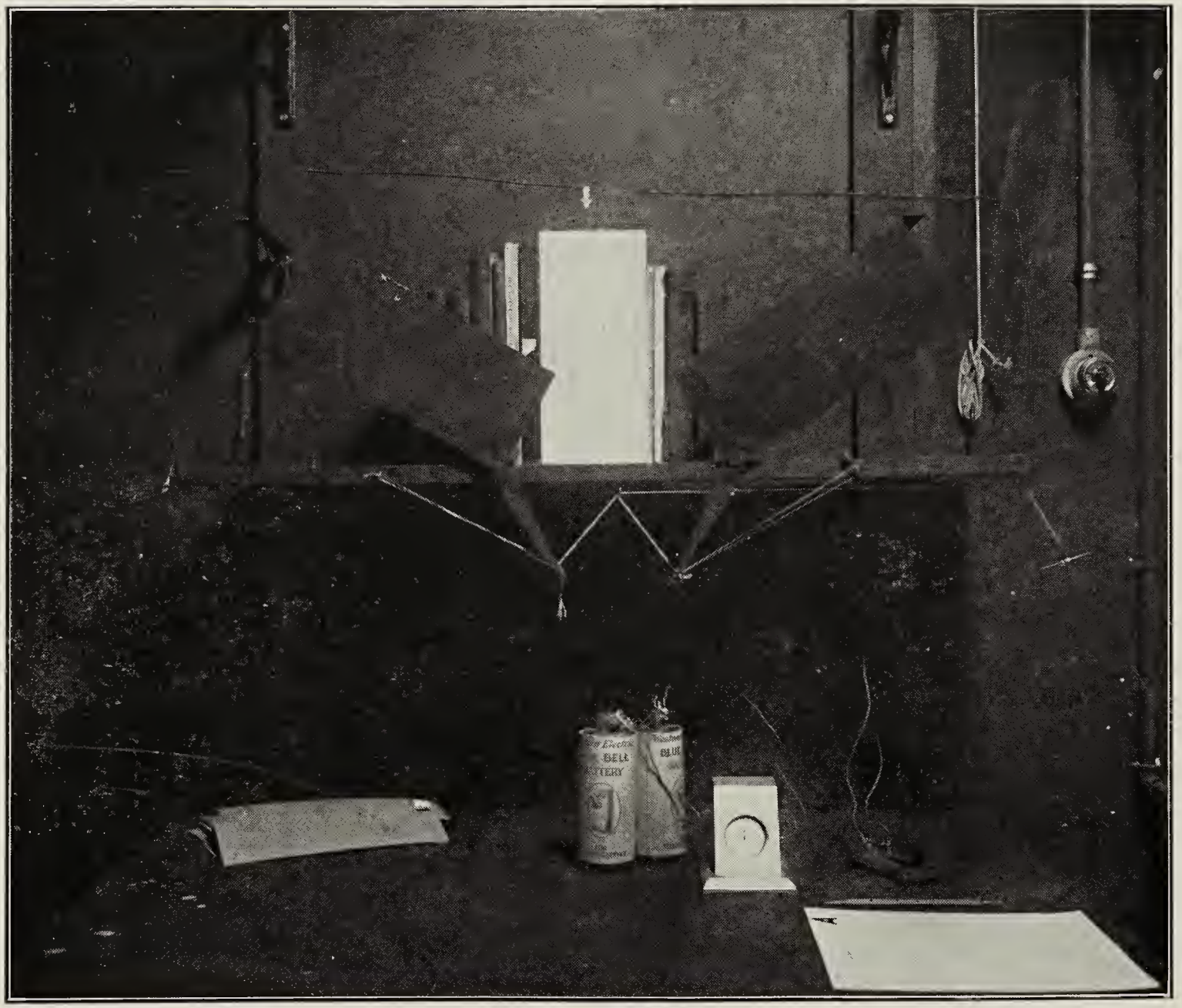


FIG. I

In front of the shelf a double scissors-like shutter was provided which would expose to view the entire length of the "dummy" and some of the neighboring books without suggesting by its own movement the direction to be followed in reading. Later in the experimental series, the "dummy" was replaced by a device that would hold six or more titles at once, the shutter was opened through an arrangement of strings, elastic bands, and pulleys, operated by the experimenter. An electrically operated stopwatch was used to time exposures and reactions of observers. The entire shelf and shutters were painted a dead black and the room was lighted uniformly by means of an artificial "noon-day" light of high power. In another part of the experiment a head rest was provided. The observer was uniformly three feet in front of the title to be read. A pointer was affixed to a wire-frame to indicate the approximate place at which the titles were to be read.

Procedure: The experimental investigation began early in 1919 and lasted through the summer of that year. The observers, fourteen in number, were selected from among the advanced classes in experimental psychology. They were given instructions to read aloud as clearly as possible, and at their normal speed, the title that was to be presented to them. Introspections were recorded in about half the cases.

The series were divided, in the first group of experiments, so that 36 observations were taken with the titles at the level of the eyes, the same number below, and an equal number above the level of the eyes. It was soon found that the reading of a single title could not be accurately timed under the conditions of the experiment, so, for the most part, cards were made up of from four to eight titles running in the same direction, to be read consecutively, and the resulting time divided by the number of titles read. In the second group of experiments the observers were asked to walk from a spot about five feet to the right or to the left of the shelf and then walk towards the shelf, on the assumption that entrance to the room constantly from one direction would itself influence the observer *to favor* one side or the other.

In a final group the observer's head was placed in a head-rest which prevented movement to either side.

In each case, after the observer had taken his position the experimenter would open the shutter and release the stop watch with the same movement. When the reading was completed the watch was electrically stopped. The arrangement of the cards was determined by a chance order but care was taken that each presentation to the left, that is from bottom to top, was at some place matched by a presentation of the same material in the opposite direction.

As a result of our questionnaire (see below) we have also the unpublished records of some class experiments conducted by Professor A. P. Weiss of the Ohio State University and of a few individual experiments performed in the same laboratory. Some disjointed printed material mounted on a board and 200 words in length was presented to the observer to read aloud as rapidly as he could accurately do so. The time was recorded in seconds. Both the copy and the observer were given a variety of positions, *viz.*, (1) the copy upright and the observer upright, material read from left to right; (2) copy upside down, observer erect, material read from left to right; (3) copy turned to the left 90 degrees, observer erect, material read from bottom up; (4) copy turned to the right 90 degrees, observer erect, material read from top down; (5) copy turned to the left 90 degrees, observer bending to the left 90 degrees, material read from bottom to top; (6) copy turned to the left 90 degrees, observer bending to the right 90 degrees, material read from bottom to top; (7) copy turned to the right 90 degrees, observer bending to the left 90 degrees, material read from top to bottom; (8) copy turned to the right 90 degrees, observer to the right 90 degrees, material read from top to bottom.

The time values in these series do not present significant differences. For example, there is a difference of only two seconds in the median values of series 5 and 8, which series approximate most closely the question raised in our problem. It is significant, however, that out of fourteen observers who expressed a preference for one of the last four positions, eleven preferred position 5, two preferred position 8, and one was doubtful regarding 5 or 8. This apparently gives a favorable vote to the position which involves bending to the left and reading material from bottom to top.

Observations were also taken by five persons, in the library, to ascertain which way the head and body are bent when reading book titles in the stacks. In 187 cases observed, 29 per cent. bent voluntarily to the right, while 71 per cent. bent voluntarily to the left. Obviously these results should be suitably qualified by reason of the fact that they were not obtained under experimental conditions. Such factors as differences in illumination, position of books as regards the aisles, just previous position of the observer, height of books from floor and similar circumstances, clues to which could

very likely be obtained in individual reports, were not taken into account. The figures speak for themselves only in the mass. And yet, of course, when so taken they are very good indicators of a possible conclusion.

Results: As far as our own statistical records go, the time values and also the number of times which indicated a faster reading for one or the other direction show no general significant features. The reading times indicate the least variation when comparisons are made in the values obtained in reading upwards with those obtained in reading downward. There is, of course, large individual variation with the observers. The only figures that seem to have any relevancy are the total times that the series was read faster in one direction than in the other, and there, only as regards that phase of the problem which took into account the height of the shelf from the floor and the direction from which the shelf was approached. When the shelf is above the level of the eyes the bottom to top direction is most favorable, whether it is approached from the right or the left, although slightly more so when approached from the right. In 69 per cent. of the cases the times were faster from bottom to top and in 31 per cent. they were faster in the reverse direction. When these percentages are factored into values which signify the direction from which the shelf was approached, we find that the value for the approach from the observer's right reaches as high as 77 per cent., whereas that for the approach from the observer's left decreases to 59 per cent. At the level of the eyes there seems to be no appreciable difference in time values or in the number of times either direction is read faster. This is markedly true in the series in which the head was fixed. In the series in which the shelf was placed below the level of the eyes, there is a slight preference for the top-to-bottom direction as expressed in the number of times this direction was read faster. The percentages are: .57 for the top-to-bottom and .43 for the reverse direction. When these values are factored into figures which indicate the direction of approach, they increase so that the top-to-bottom direction is .64 when approached from the right and .49 when approached from the left. The writers, however, wish

to emphasize the fact that there is so slight and sometimes so inconsistent a difference in the actual time values for reading in one or the other direction, that comparatively little evidence in favor of printing titles uniformly either from bottom to top or from top to bottom is forthcoming from the mere statistical account of our investigations and more stress is to be placed on the observers' comments. Stated in other words, as a matter of sheer efficiency gauged in the time actually spent reading the titles, we find no noteworthy or uniform difference.

Something must be said, nevertheless, both for the mental attitude of the reader as regards his reaction to the confusion which now obtains respecting the printers' and publishers' practice and for the time that may actually be lost before the reading of the title is begun. This latter value was, of course, omitted entirely from our calculations. It is a value hard to obtain, and compares with the well-known "fore-period" of the reaction-time experiments.

The introspective comments in almost every case diclose prominent kinaesthetic sensations localized about the head and neck and occasionally in some of the larger trunk muscles. These appear before the reading is begun and last through the reading period. Frequently they are affectively colored, and unpleasantly so before a sudden change of position, *e.g.,* when the direction of reading is opposite to that anticipated. To quote:

"Slight tendency to tilt head according to reading of card. Tendency to read from bottom to top of card. Cards blurred more or less by fixed position—eyes used to slight tilting of head to ease strain of reading."

"Pleasantly colored sensations while waiting for opening of shutter—desire to begin quickly. Unpleasant emotion when top of sheet is at the right, pleasant when at left (bottom to top)."

"When the cards were perceived to read bottom to top, there was a kinaesthetic sensation in the neck resulting from turning the head to the left side."

"When cards were shown there was an involuntary kinaesthetic twitch in the muscles of the eyes toward the upper right-hand corner in all cases."

"When the cards were exposed there was a kinaesthetic sen-

sation of movement of my eyes to the lower left-hand corner of the cards. If I perceived that this was the point at which it was easiest to begin reading (if the lines ran upward) I began reading at once without difficulty. In case they ran downward, however, it was necessary to focus my eyes at the upper right-hand corner."

It is clearly seen that the mental accompaniments of the reading must be taken into account. They parallel the actual reading in many cases without disturbing the reading time. In most cases, however, there are kinaesthetic adjustments made in anticipation of the reading and if these are later to be altered an unpleasant affection results which seldom disturbs the reading. It is barely possible that if the experiments were to be continued and the time accurately measured from the opening of the shutter to the beginning of the reading, especially if the reading were done through an electrically controlled lip-key or the Dunlap voice-key, significant time values would result for the two directions.

Questionary Series

While the investigation was proceeding, some 92 answers to questionnaires were received, about one-half of this number from colleagues on the faculty of the University, and the other half from psychologists selected from the American Psychological Association. A copy of this questionnaire follows.

Dear Sir:

Librarians and others are just now discussing the problem of printing titles lengthwise on the backs of periodicals and thin books. Some maintain that the more readable direction is from top to bottom; others take the opposite view. The question has been referred to us for experimentation and the investigation is now under way.

In connection with the experimental results that we are obtaining, it is of interest to us to learn the personal opinions of those who are accustomed to the use of such material. For this reason we should very much like an expression of your reaction to the following statements:

(1) When publications of this sort are in position on a book-shelf among other miscellaneous books, I find that I am inclined to read the title more readily when printed from { top to bottom / bottom to top }

(2) There are circumstances, however, which may modify this inclination; these are:

(3) When inscribing titles along the backbone of reprints inserted in covers, I usually write them from { top to bottom / bottom to top }

(4) I am normally { right-handed / left-handed }

A number of affiliated questions are frequently raised in this connection. Among them are: (a) Is the head generally inclined more readily to one side than the other or are there frequent variations? (b) Does the predisposition to read lines from left to right and from top to bottom affect the question raised? If you care to discuss these questions or related questions on the back of this sheet in addition to your answers to previous questions, we should be glad to hear from you.

Thanking you for your coöperation, I am

Very truly yours,

(Signed) CHRISTIAN A. RUCKMICK.

When the votes are counted without reservations we find 48 in favor of reading from bottom to top and 34 in favor of the reverse direction. Fifty-three were in favor of writing the titles on the backbones of monographs from bottom to top, and 30 in the reverse direction. In this connection it is interesting to note that while some indicate a preference in reading from top to bottom they state in reply to the third question that they write in the titles in the reverse direction. On the other hand, not a single case appears in which the bottom-to-top method of reading is combined with the habit of writing titles from top to bottom.

If space permitted it would be instructive to publish all of the individual comments made in reply to questionnaires. Some have already been summarized in our introduction to the problem. There seems to be without question an individual difference in the matter, with the majority of habits formed in favor of tilting the head to the left, making the bottom-to-top reading probably the more agreeable one. At best many consider the practice a necessary evil and, as one well-known psychologist has it, one would "vastly prefer any kind of makeshift in the way of horizontal abbreviation which would obviate the necessity of straining one's eyes to the required position in order to read the vertical type."

Summary and Conclusion: It will be seen from our studies that, so far as the rapidity of reading is concerned, there seems

to be no pronounced difference in favor of one direction or the other. With certain factors added, such as height of shelf, position of the book to the right or to the left of the observer, individual habits, and above all, individual preferences, especially as these are emphasized in our verbal reports, then there appears a significant margin of choice in favor of the bottom-to-top direction. Further experiments investigating the period just before reading and possibly also the period after reading should be planned and introspections recorded.

Replies to the Questionary

Since the comments, theories, and explanations that were returned with the printed questionnaires would be lost in any statistical treatment of the results obtained, it is better to let them tell their own story without modification. Accordingly there is appended a series of such discussions selected from the entire set of returns. Only those have been omitted whose statement of the point is found in some other quotation and is usually therein better expressed. Many of the statements come from prominent psychologists and other widely known men of science; but because it is doubtful whether we could properly attach their signatures to material thus obtained, we are presenting them only on their intrinsic merits.

"I try to have the legend on the 'backbone' of every publication that is not wide enough to print across printed to read from top to bottom. As my function is more that of the maker of the periodical than of the user or the librarian, I am interested in having all such arrangements meet the convenience of the persons who use them.

The best summary of the argument for top-to-bottom readings is found in H. P. Ward's "American College Catalog," page 79:

"Although some college catalogs are made with the backbone title reading from bottom to top, many are properly printed with the title reading from top to bottom. The reason for this form is that when the catalog is laid flat on a table or desk, front side up, and other catalogs are placed on top of it, it is possible to read the backbone title without taking it out of the pile. The best explanation of this matter would be a glance at a pile of old magazines. It would be far better if the few catalogs whose backbone titles read from bottom to top could be treated as the great majority are, with the backbone title reading top to bottom, for the sake of uniformity of appearance in the Exchange Catalog Shelves of hundreds of institutions and libraries. The librarian customarily passes along a shelf from left to right. reading from top to bottom. He is entitled to some consideration.

It seems to me that the question of uniformity is the important one here."

"When I started in to print the *Review* I was perfectly shocked to find that the printer had printed the title on the back of the magazine from bottom to top. It looked all wrong to me and yet the printer declared that that was the usual custom. I told the usual custom to be hanged; the *Review* was going to suit my aesthetic taste, and so it follows it from top to bottom."

"(a) I find I incline my head more readily to the left than to the right. (b) Predisposition to read from left to right favors printing titles from the top toward the bottom because when the book is laid flat on the table or book-shelf with the front cover up, the printing on the back is then right-side up and reads from left to right. Perhaps the disposition to read from top to bottom also favors the printing of the title in this way.

In spite of the considerations mentioned in (b), I prefer the reading from bottom to top. In this connection, it may be worth while pointing out that this is the practice of the government printing office—at least it is so in such bulletins as fall under my eye as I write this."

"I return herewith your outline and in this connection wish to say that I have gone over my private library including many thousand pamphlets and as nearly as one can determine by a rapid examination the following is true of the publications which have printed labels on the backbone. Older journals printed in America have the label running from bottom to top, where the same periodical in the last dozen years has changed its habit. The same is true of government publications issued in the Philippines. However, those printed in Washington, namely the Geological Survey and the United States National Museum, have consistently followed the rule of beginning the line at the bottom and printing towards the top. All of the English periodicals to which I referred have the titles on the backbone started at the bottom and reading towards the top. All recent French (one exception), German (one exception), Italian and Russian publications make the title read from the bottom towards the top. In old German publications this seems to have been the exceptional method and of the few I had almost all had the title printed in the opposite direction."

"Recent scientific publications of medical character printed in this country have the backbone title reading from the top towards the bottom."

"I found on attempting to study the shelves, I approached them always in such a way as to stand at the right and then it was natural to read from the bottom towards the top following direction from left to right. When I tried to stand at the left in which it was possible to read easily from the top to the bottom, it required considerable effort to push my head into the proper position and it was not natural to handle the books because in separating them one always looked at the back of the pamphlet, whereas in approaching from the right one could pull the books apart and the eye fell naturally on the front of the pamphlet. It seemed to me that there was not any difficulty in inclining the head towards one side rather than towards the other. I am not sure that habit is the factor involved but my experiment lead to the distinct feeling that one way was natural and the other not."

"In a filing drawer where pamphlets are grouped back up, it is very inconvenient to have the title read from top to bottom. Come over to see my library some day."

"I believe this habit of reading from below upwards may be connected with that of writing with the paper tipped upward at the right. It would be very awkward to write with paper tipped downward at the right; that would demand a cramped inward movement of arm, whereas inclining paper upward at right gives a free outward movement of arm."

"In reading the titles of volumes on shelves printed lengthwise of the volume I always stand on the left side of the volume and the head is inclined to the right side. I find it difficult to stand on the right side and to incline the head to the left. All geological publications of United States and Canada, British colonies and nearly all British print from bottom to top. Perhaps this may be reason for my preference."

"It would be inconvenient to write from bottom to top as I hold book in left hand and it requires less energy to turn it into position to write bottom-top than top-bottom."

"The 'psychology' of it with me appears to be that when a lot of such things are shelved in alphabetical order by author (or title) I seem to be reading them *down* the pile,—much as I would continue the alphabet *down* onto another shelf; so they resemble a series of alphabetical titles on a printed paper which I read from top to bottom far more readily than I should if they began at the bottom with A and wound up with Z on the top line."

"All narrow columns such as the headings to columns in tables read from top to bottom. This holds true even though the headings are spelled out by single letters. The habit is thus formed of reading downwards. This may seem inconsistent with the common practice of placing tables and cuts that run lengthwise of the page so they read from the bottom as the left hand side of the page. But to read such tables we must *turn the book,* and the most natural method is to turn it 'clock-wise.'"

"My head inclines more readily to the left. I attribute this to the fact that I always have the light come over my left shoulder, and hence usually read with head inclined a little to the left. Likewise, right-handed people write with heads bent to the left."

"I find also that I have had to put labels on a very considerable number of small slim botles and in every case it reads from bottom to top. I naturally pick the bottle up in my right hand by the top and swing the bottom to the left, so the bottom to top label is easily read. I have counted 20 booklets and pamphlets here which have titles lengthwise on the back—all as prepared by the printer. Eight have titles bottom to top and 12 top to bottom. To my mind there is little difference after we get used to either, but uniformity is desirable."

"Titles of books are usually printed at the top, often back of the volume.

The beginning of the lengthwise title should be near the same line as the cross-wise title."

"In searching for books on a shelf, using titles, a man usually works from left to right, and so likes to catch the clue word of a 'back-bone' title on the same level as other titles, without fumbling lower down."

"In my own case, I am quite positive in this matter. Even in so small a matter as inserting a word I write it tilted *thus* . When going down a shelf of books you naturally follow, instead of backing along, as you cannot do when you read from the bottom up."

"Personally I have a slight preference to turn my head to the left in looking at such titles which would mean printing from bottom to top. I arrange MSS. in folders on that principle. I am right-handed. On the other hand, I am *strongly* of the opinion that all such titles should be printed from top to bottom for the following reason. Unbound magazines are more frequently laid flat than on edge. It is absurd to lay the magazine face down; and it looks equally foolish to see the title upside down on the back. The edge titles of unbound magazines should therefore be printed from top to bottom. Why have one rule for magazines, another for books? A thin book is often laid flat and it is much simpler to have the title page uppermost and the back title upright. On the ground of simplicity I should recommend very urgently that one uniform rule be adopted, namely, top to bottom."

"The only justification, in my opinion, for the bottom to top method of printing titles is the stacking of books and reprints horizontally, rather than vertically, and even this is doubtful value. For, when books are kept horizontal, the book to be consulted must be turned over through an angle of 90° before the title page is readable."

"A right-handed person commonly writes with the lines sloping from lower left to upper right of the visual field to accommodate his forearm movements. Never the opposite slope as far as I have observed. This is reversed for left-handed persons. This gives a considerably greater familiarity to upward reading lines to the majority of persons. My practice is, wherever I am not otherwise constrained by positions on a ladder or other considerations, to turn pages with the right hand and hold a book with the left. This naturally leads to taking a book from the shelves with the left hand. As a matter of economy of effect this is worth encouraging by upward reading titles. The main point is uniformity. We can readily adapt ourselves to either way of printing. Lack of uniformity is disturbing and fatiguing."

"If I incline, not *only* in order to *read,* but *read* and *handle* at the same time, I incline to the left because I handle with the skillful right and support the body with the unskilled left. The conditions are so complex, that it is of little average advantage to use one method of writing titles rather than the other. We might as well decide it by tossing up a coin. But it is

of the greatest advantage to have all titles written the *same* way. Maybe one style is already preponderant. Then *that* should be *generally* adopted."

"If a book is lying before you, right side up, opening at right, a title printed lengthwise on back can be read by moving head and tipping to left, with a slight raising of the back of book, *if* title is printed from bottom to top. But if printed from top to bottom, greater movements (changes of position) are required. This seems to me to be the only fact indicating which is the 'normal' method of printing. Either method would be satisfactory, if made universal. One would as easily form the habit of looking one way as the other along the shelf. I prefer, however the 'normal' arrangement of reading from bottom to top."

"If I were sitting in front of a bookcase at some distance I would prefer that the titles be printed from top to bottom. If I were in front of a bookcase and passing from right to left, I would prefer that the titles be printed from bottom to top. If I were to pass from left to right I would prefer that the titles be printed from top to bottom. I would naturally pass from right to left. If I were sorting through a pile of reprints placed in front of me, top cover up, I would prefer that the titles be printed from bottom to top."

"In reply to your inquiries of February 12th, I write to say that the practice to which you refer always fills me with suppressed rage, for unless the book concerned happens to be at the level of my eyes, I find myself attempting to stand on my head in order to compass the necessary reading. My impression has been that I find this process less distressing when the title is entered from top to bottom than when in the reverse form; but either practice has always seemed to me an invention of the Evil One. I vastly prefer any kind of makeshift in the way of horizontal abbreviation which will obviate the necessity of straining one's eyes to the required position in order to read the vertical type. Although I have never made careful experimental study of the matter, my general impression has been that if books were on shelves below the level of my waist, I would prefer to have the titles run from the bottom up. With the shelving in my own library this occurs less frequently than the converse way with the books above my waist and perhaps explains my supposed preference for the title running from the top downward."

"My head inclines more easily and automatically toward the left. I read more comfortably with a left-hand inclination. The usual flat position of a book held in front of the face, title page up, brings the reading on the back from bottom to top."

"I slightly prefer inclination of the head to the left, but I am inclined on the other hand to approach a book shelf from the left and this necessitates the reading of titles from top to bottom. Undoubtedly, the habit of reading from left to right affects the situation. I recently noted that when titles of books are printed lengthwise of a page, the orientation differs. There should certainly be uniformity of practice in this connection. I pre-

fer to have the lines begin at the bottom of the volume extending thus from bottom to top. Evidently the conditions which influence me in this case are somewhat different from those in the case of the title on book or volume. If instead of reading titles on a book shelf, I lift a volume from the table to read lengthwise title, I very much prefer to read from bottom to top."

"In reply to your questionnaire with regard to printed titles on the backs of books and periodicals I beg leave to say that I find on examining my preferences that I prefer a title that reads from the bottom to the top. I find that my method of approaching the bookcase is such that I naturally tip my head toward my own left hand and stand in such a position that I read up rather than otherwise. I did not realize that I had the preference as fully as I find that I have. I am quite right-handed and do not know of any general considerations that would have prompted the development of this habit. I call your attention to one fact, well known to oculists; there is a tendency for the head to be bent so as to bring the plane of normal fixation of the two eyes into a position where there will be the least muscular strain on the eyes. The neck muscles in this way coördinate in making eye adjustment."

EXPERIMENTS IN SOUND LOCALIZATION[1]

By C. A. Ruckmick

It is not necessary to give the historical background of the problem since that has already been given in several recent studies.[2] It should be said, however, that new interest was aroused concerning the investigation of sound localization because of very urgent questions that arose in connection with the war, questions which indeed made both the physicists and the psychologists realize the dearth of material on the subject. For example, practically nothing of scientific value had been done in either of these sciences with the problem of sound localization through mediums other than air. The need for accurate and rapid detection of sapping activities on the part of the enemy and the necessity of localizing with equal precision and speed the presence of enemy submarines made it clear that we should know facts concerning the localization of sound through the solid earth and through water, in addition to more detailed information concerning the localization of aeroplanes in flight. Accordingly, both physical and psychological scientific societies undertook, through specially appointed committees to make further specific investigations into these matters.[3]

[1] These experiments were undertaken by a group of students doing advanced or graduate work in the Department. The experimenters who were responsible for various sections are: Miss Harriet Anderson, Miss Hilda Kohl, Miss Anna Polkowski, and Mr. A. W. Gross. To these and to the many observers, the compiler of the results wishes here to give suitable recognition.

[2] One of the best summaries of the literature is given in Klemm, O., Untersuchungen über die Lokalisation von Schallreizen, *Arch. f. d. gesam. Psychol.*, 1918, xxxviii, 71-114.

[3] Besides coöperation with the physicists, through consultations and the lending of apparatus, the author undertook to contribute his share of the work of the Committee on Audition, appointed by the American Psychological Association, by supervising three or four pieces of research in this field.

I

Without going into the controversy regarding the question of the binaural ratio, we attempted to indicate the influence of relative intensity in the localization of sound under conditions that should guard against certain psychological errors of which experimental research on the part of physicists is not always free. In particular we attempted to duplicate, in principle at least, the work of Myers and Wilson,[4] two investigators who seem not sufficiently to have considered errors of this sort.[5]

Apparatus: We used two quiet rooms in the psychological laboratory, one of them a large dark room (Fig. I). In the outer room, a large wooden box enclosed a Stern variator (No. 2, 150 vd. to 300 vd. Opposite the lip of the instrument a large eight-inch tin funnel was inserted, with its outer edge bound by rubber and directed towards the variator. Its narrow orifice was extended by means of rubber tubing ¾″ in diameter to the operating table in the inner dark room. The Stern variator was set at three different pitches in the course of the experiment, 193 vd., 200 vd., and 217.5 vd. Rubber tubing from the compressed air service piped into the building conducted air through a reducing valve to the intake of the Stern variator. The inside of the box was thoroughly stuffed with cotton waste and the outside of the box was wrapped with deadening material made of seaweed. All openings between the two rooms were tightly sealed, and several observations were made to determine whether any sound could be heard in the inner room when the valves were closed and the Stern variator was operating. The large rubber tubing passed through a carefully packed opening in the wall between the two rooms to two sliding valves. Before coming to these valves the tube bifucated through a glass Y. The sliding valves were so arranged that the full diameter of the

[4] Myers, C. S., and Wilson, H. A., The influence of binaural phase differences on the localization of sound, *Brit. J. of Psychol.,* 1908, ii, 363-385.

[5] The author would like to see some of the experiments on the intracranial localization of sound repeated under rigid conditions. It seems remarkable, for example, that (in our experiments) no localization was reported, as referring to either ear, or to any position "in the head."

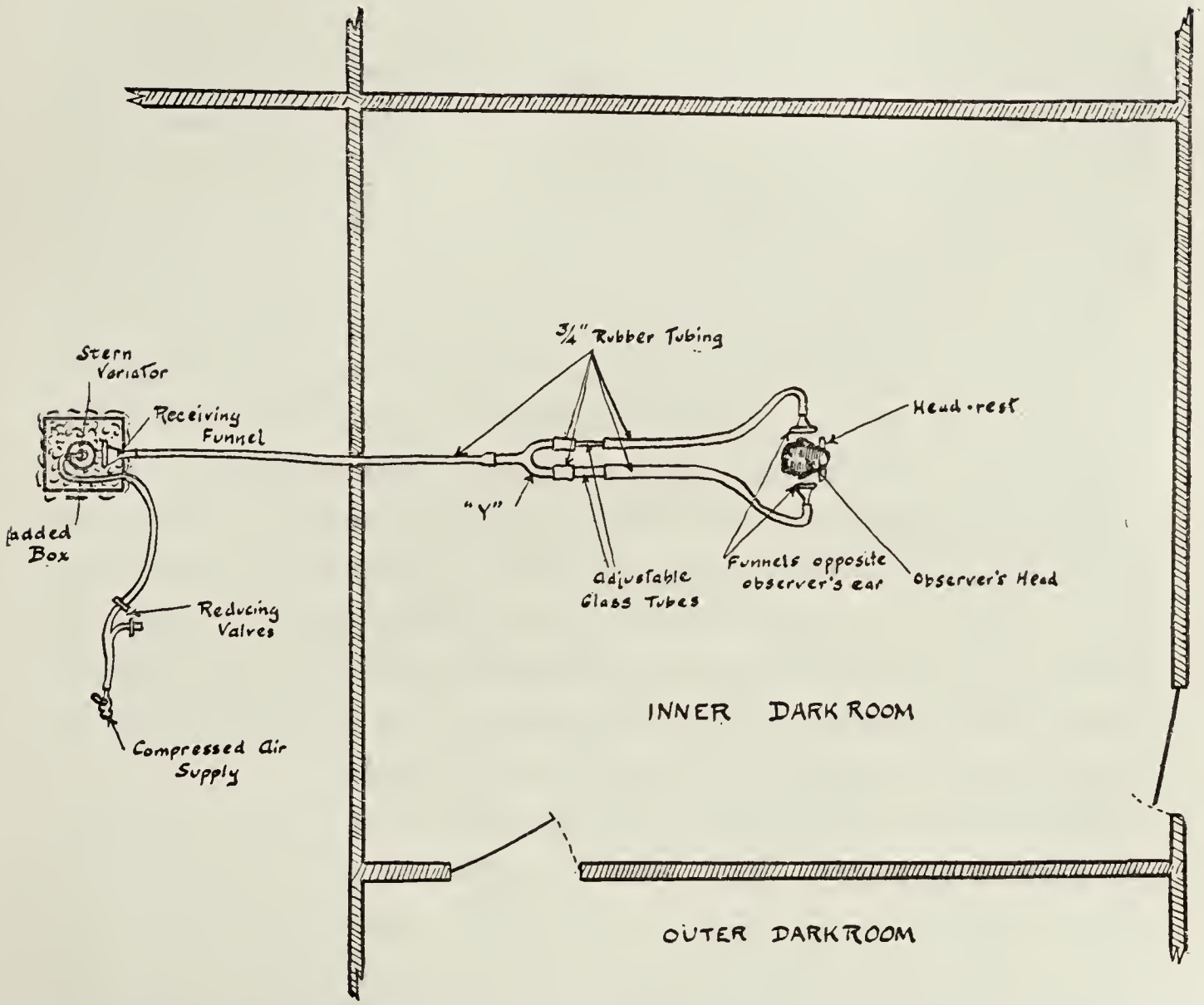


FIG. I

tube was maintained but could be reduced by the experimenter. Since the question arose in the preliminary series whether a valve of this sort might not, when partly closed, produce eddies in the current of air, later in the main series we used glass tubes of nine different diameters, .15, .2, .4, .42, .45, .6, .65, .9, and 1.1 cm. These were always used in pairs, one on each side of the Y. From this point on a pair of ¾″ tubes was continued for about 10′ and ended each in a small 2″ tin funnel, the outer edge of which was bound in rubber. The funnel on each side was held in a standard opposite the ear of the observer, and the observer's head was fixed in a head-rest.

Procedure: It must be understood, of course, that none of the observers ever knew, in the course of the experiment, the arrangement of the apparatus or the nature of the problem. The observer was led blindfolded into the dark room and great care was

exercised by the experimenter that, as he sat down and inserted his head in the head-rest, his head should not touch either funnel. The four observers who took part in the experiment never realized that two separate sounds were coming to their ears, and, in fact, when subsequently told of the conditions, they could hardly be persuaded to believe the situation to be as described.

When the observer had been seated he was instructed to visualize a sphere with his head at the center and the zero point directly in front, and to keep clearly in mind the quadrants to the left, right, above, and below. He was further instructed that a sound would be produced in the room and that he was to localize this sound in terms of the imaginary sphere and by such designations in the sphere as would enable the experimenter to record the results. In some cases these symbols were adjusted to the preferences of the observer. He was also required to estimate the distance that the sound was away from his head.

The reports of the Obs. were taken down by the experimenter on a mimeographed blank which designated the number of the trial; date; observer; position left, right, above and below; intensity; distance; and "remarks." There were over 650 trials recorded and results tabulated. In the preliminary series, nine positions of the valve were marked on each side, and a haphazard series was arranged in advance to include the various combinations of these positions. The same plan was followed in the main series, in which the glass tubes were used in place of the valves.

It is clear, then, that since the length of the tube on each side was kept precisely the same, and since the only change introduced was a change in the opening through which the sound waves passed, there could be no modification of the wave save amplitude, a modification calculated to produce a difference of intensity for the two ears. This is especially true of the second part of the investigation in which eddies were avoided.

The experiments were conducted during the academic year of 1917-1918 and during the summer session of 1918, in the Psychological Laboratory at the University of Illinois. The observers were well trained advanced students in the department.

Results: In the preliminary investigations the results do not show marked correlations with the different positions of the valves unless the opening on one side was much greater or much less than on the other. We find, however, that the summation of apertures had a marked effect on the distance of the sound. When both openings were large the sound was reported as much nearer than when they were small. The modes varied respectively from a distance of 20 ft. in the case of one-ear presentations to a distance of one foot in the case of two-ear presentations.[6] Another way of illustrating this fact lies in percentages obtained for loud sounds, *e.g.*, 60% of the two-ear presentations were reported as "loud" or "very loud," while only 42% of the one-ear presentations were so described.

While our results on the matter of the binaural ratio are preliminary and tentative, a study of the individual tables points to the conclusion that there is a tendency to localize a sound more and more toward the median plane as unity in the binaural ratio of intensity is reached. The data on this point are not as yet sufficiently free from the errors of the ordinary "sound-cage" procedure to permit of precise mathematical formulation. One of the chief difficulties which confront the investigator in this field is the inability of the observer faithfully to reproduce his judgments in an imaginary sphere either by the visualization method or by the pointing method. But expressed in percentages, there are indications in our experiments that the ratio of intensities, as transmitted independently to the two ears, was the controlling factor in the localization. Eighty-six per cent. of the sounds presented to the two ears with unlike intensity were localized on the side of the greater intensity. Sixty per cent. of the sounds produced with equal intensity were localized in the median plane. It must be remembered, of course, that the fewest errors occurred when the difference in intensity was marked and that some sounds with a moderate difference in intensity were localized in the median plane. Although all the observers were tested for acuteness of hearing and fell well within the

[6] This ratio is somewhat larger than the summational effect reported by Klemm, *op. cit.*, 79, but, of course, not so precisely obtained.

normal, there was still an individual difference in the results of our experiments, due largely, it is assumed, to variations in ability to designate the localization.

Conclusion: On the basis of our results we can not, of course, speak with absolute assurance concerning the rôle of binaural intensity in sound localization. Our experiments, confined to a limited number of observers and subjected to the usual methodological difficulties, convince us, nevertheless, that no factor outside of intensity could be held responsible for the results obtained, and that the results themselves were far above the figures for chance variation. We should like to see the experiments repeated under better conditions, perhaps out-of-doors, and with a larger number of observers, but with a strict adherence to the procedure "without knowledge."

II

Investigations were also under way regarding the difference in the ability of an observer to localize sound due to the position of his body. One does not have to recall here the questions that have arisen in the literature concerning the dependence of sound localization on the semicircular canals and the muscular adjustments due to the inner ear. It was assumed that, if the reflecting surfaces in the room could be kept uniformly constant for each position, any differences in sound localization within a sound-cage would be due to these disturbances.

Apparatus and Procedure: In our normal series we placed the observer in a sitting position within the standard form of Stoelting sound-cage whose telephonic click was modified by introducing a vibrating induction coil. The telephone receiver was connected to the secondary circuit of this coil. Over 400 observations were made including the series in the sitting position. In connection with the other positions, a couch was provided and so braced that the head should be projected beyond one end and that it should then be approximately at the same point as it was when the observer was in the sitting position. An equal number of observations were taken with the observer lying on his right, on his left, on his back, and with face down. The room used

was the largest in the laboratory and, so far as possible, reflecting objects within the room were removed.

Results and Conclusions: In calculating the errors for each position we find no conclusive evidence of any marked differences due to inclination of the body. We eliminated, however, from each series the first five judgments on the ground of inaccuracy due to making judgments in unaccustomed positions. This part of the investigation also might well be repeated under better conditions and with more observers. Care must be exercised, of course, that the couch used does not itself add new reflecting surfaces of any consequence and that it is, at the same time, fairly comfortable for the observer.

THE INTENSIVE SUMMATION OF THERMAL SENSATIONS

By Annette Baron and Madison Bentley

I

When we plunge the fingertips into water there usually arises a sensation of cold or of warmth which seems gradually to increase in intensity as more and more of the hand is immersed in the liquid. From this common experience the conclusion has often been drawn that the intensity of a temperature sensation is a function of the size of the stimulated area.

E. W. Weber, who long ago cited the instance,[1] believed that the impression of temperature felt when a surface is heated or cooled was due to the summation of the intensities of the several sensations aroused. On the other hand, Stumpf,[2] arguing from the analogy with auditory sensations, later expressed a doubt concerning the actual intensive summation among sensations of cold and warmth. From his studies on tonal fusion, he concluded (1) that the intensity of a sound is not augmented but rather diminished by the presence of other simultaneous sounds; and (2) that a tonal complex is no stronger than its strongest constituent. There is, then, as he thinks, no real summation of intensities.

Now let us consider the possibilities in the case of temperature. Stumpf himself suggested three alternative explanations of this apparent augmentation of temperature as the hand is plunged deeper and deeper; (1) the confusion of spatial extent with intensity, (2) the stimulation of more sensitive regions as the hand is gradually immersed, and (3) an augmentation of sensory feeling.

Besides these three possible substitutes for a real intensive summation, Barnholt and Bentley,[3] attacking the problem in the

[1] Weber, E. H., in Wagner's *Handwörterbuch der Physiol.*, 1846, iii, 2, 553.
[2] Stumpf, C., *Tonpsychologie,* 1890, ii, 446.
[3] Barnholt, S. E., and Bentley, M., Thermal intensity and the area of stimulus, *Amer. J. of Psychol.*, 1911, xxii, 325-332.

laboratory, have suggested (4) adaptation, (5) the presence or absence of organic accompaniments, (6) the confusion of pressure with temperature, (7) the addition of individual organs of higher tuning, and (8) a difference in cutaneous conduction in large and small areas.

These experimenters applied temperature points to areas and to individual organs for cold and warmth, on the forehead, hand, and forearm. To come to a decision among all these possible explanations of the simple observation from which they set out, they devised four different methods.

The first method consisted in the immersion of the fingers to graduated depths. The larger surfaces were reported as giving more intense temperatures, thus confirming common observation; and, moreover, the introspections showed the judgment to be really based, in part, upon intensive differences.

In order to simplify the conditions, a second method was employed in which a series of five flat-ended brass cylinders with diameters ranging from 1.4 cm. to 3.3 cm. was applied to the skin. Again the larger surfaces led to reports of more intensive colds and warmths.

But to make sure that mere size of the stimulated area was not responsible for intensity, a third method introduced comparisons between graded areas of high and low sensitivities. The same cylinders were used under the procedure of "paired comparisons." The discovery that a small area which returned a vivid, bright cold or warmth could be made equal, for sensation, to a large area of low sensitivity, made it evident that not extent but tuning was the primary condition of the intensive judgment.

The factor of thermal conduction through the skin was disposed of under the fourth method. Differences of conduction were eliminated by stimulation of individual organs chosen at such distances from each other as to eliminate conduction of the stimulus through the dermal tissues. First, these temperature organs were compared with each other (1) in pairs and (2) in setting one against two others. There was found a considerable difference of tuning between the different organs. In the second

part of the final method, the experimenter observed whether reenforcement of one sense-organ by another occurred. The thermal sensation produced by stimulation of a single organ was compared with the sensation resulting from the stimulation of this same organ plus another organ of higher or lower tuning. The two organs seemed to have an intensive advantage over one only when one of them was superior in tuning. That is to say, no convincing evidence of summation was found; instead, the intensity of the sensations seemed to be determined only by the most highly responsive spot under stimulation.

Finally, Barnholt and Bentley demonstrated, by the use of artificial membranes simulating the skin, that the large stimulus may influence intensity of sensation by the mechanical process of cooling the skin under conduction. The skin is actually colder under the extensive stimulus than it is under the stimulus of smaller extent.

II

It has seemed desirable to repeat some of the methods of this experimental study, partly for the sake of verification by a larger number of observations, partly to modify the procedure in certain important respects, and partly also to consider the divergent results of another research which will presently be described. The following methods were adopted.

Method A. Four organs of different tuning for cold were mapped upon the volar forearm within an area 2 cm. square and designated by letter, "w" (weak), "m" (moderate), "i" intense), and "I" (very intense).[4] They were stimulated with blunt-pointed temperature cylinders kept at 0°C. The procedure was as before (Method IV, above), except that one organ was kept throughout a whole series, first being compared in both temporal orders with each of the other organs taken by itself, then with two of them given simultaneously, then with three, but never with itself.

In Table I the letters A, B, and C denote observers, and Tl. is

[4] A long preliminary series served to determine the decided graduation in tuning of the four organs selected.

TABLE I

		m			i			I		mi			iI	mI		wI	miI		
	Obs.	G	L	=	G	L	=	G	=	G	L	=	G	G	=	G	G	L	=
w	A	3		1	3	1		4		4			4	4			3	1	
	B	3	1		3	1		4		4			4	4			3	1	
	C	3		1	4			4		3		1	4	4			4		
	Tl.	9	1	2	10	2		12		11		1	12	12			10	2	
		wi			i			I					iI			wI	wiI		
m	A	4			2		2	4					4			4	4		
	B	4			3	1		4					4			4	4		
	C	4			4			4					4			4	4		
	Tl.	12			9	1	2	12					12			12	12		
		wi			wm			I						mI		wI	wmI		
i	A				3		1	3	1					4		4	4		
	B				3		1	4						3	1	4	4		
	C					3	1	4						3	1	4	2	2	
	Tl.				6	3	3	11	1					10	2	12	10	2	
		wi			wm					mi							wmi		
I	A	4			3		1			4							3	1	
	B	1	3			4				3		1						3	1
	C		3	1		4					4						3		1
	Tl.	5	6	1	3	8	1			7	4	1					6	4	2

the total for each set. The entire number of comparisons is 264. The letters at the left of the table indicate the cold organ —weak, moderate, intensive, very intensive,—which is compared with one or more of the other organs, written horizontally across the table. Thus "3" written near the upper left-hand corner means that Obs. A three times judged the cold from the organ of moderate tuning (m) to be greater (G) than the cold from the weakly tuned organ (w, at the left). The relatively large number of "greater" (G) judgments where each organ is compared with *one other* of *higher tuning* (w with m, i, or I; m with i or I; etc.) clearly establishes the wide and constant difference in sensational intensity with which our four organs responded throughout the experiments. An inspection of the results will make it apparent that the member of the comparison which contains one or more ogans of high tuning is nearly always judged to be more intensive (G) than the other number.

In order to guard against the variable errors of time,—expectation, habituation, fatigue, and the like,—the one-to-two and the one-to-three judgments were all repeated with all Obs. in a mixed order of presentation with a selection by chance. This

control-series numbered 128 additional comparisons. It led to 100% of "greater" judgments (1) when 2 or 3 organs of higher tuning were compared with 1 weaker, and also (2) when the single organ stood intermediately with respect to 2 or 3 others. In those cases where an intensity from high tuning was compared with two intensities from low tuning the L-cases were only 4 out of 24. These results, then, are as unequivocal as the others. Their significance will be more apparent from Table II, where both sets are combined, with the omission of the one-to-one comparisons. Here the intensity from one organ is always compared with 2 or 3 organs of different tuning. The G, L & = judgments are always set down in terms of the plural organs.

TABLE II

		G	L	=	T'ls
	2 organs of HIGHER tuning	79	0	1	80
	3 organs of HIGHER tuning	19	1	0	20
1 organ of HIGHER	and 1 organ of lower tuning	78	0	2	80
1 organ of HIGHER	and 2 organs of lower tuning	18	2	0	20
2 organs of HIGHER	and 1 organ of lower tuning	20	0	0	20
	2 organs of lower tuning	25	34	21	80
	3 organs of lower tuning	14	4	2	20

The Table reads thus: the presence of one or more organs of higher tuning, leads, in the comparisons, to the judgment "greater intensity" in 214 out of 220 cases; while the comparisons with organs of lower tuning alone leads to "greater" judgments in only 39 out of 100 cases, and in only 16% in the second, most reliable, set of experiments. But if the *number* of temperature organs stimulated, instead of their *tuning,* had determined the judgments, then there should have been as many G-cases in the last hundred as in the first hundred; and again, these cases should have run with the plural organs in the middle group.

These last figures show even more clearly than the corresponding results of Barnhold and Bentley the essential dependence of intensity upon tuning. There is, however, left the fact that plural organs of *lower* tuning than the standard do give rise to G-cases in larger number than the L-cases from plural organs of *higher* tuning. There is still left the implication that number does play a minor rôle in the determination of intensity. This

implication becomes explicit in the experiments of Siebrand, which were published[5] at about the same time as the Cornell Experiments. Although Siebrand did only a few experiments and although he neglected the factor of tuning, we have thought it well to repeat his methods under the necessary precautions.

Employing the flat end of a brass rod of 12 mm. diameter, Siebrand (p. 213ff) stimulated in succession two areas of like extent within which he had previously determined the number and position of the cold organs. The surface including the greater number of organs he and his Obs. sensed as colder in 80-90% of his (60) cases. In concluding that intensity depends upon number, however, he overlooks the fact that the greater the number of receptors the greater the chance of including at least one organ of high tuning. Our own experiments follow.

Method B. i. Comparisons between two areas of the same extent. A square area, 2 cm. x 2 cm. was laid off upon the volar forearm of the observer. The number and position of the cold-spots were determined by means of blunt-pointed cylinders. When the organs had been accurately localized upon the square their tuning was established by comparisons. Four degrees of tuning were used; w, m, i, and I. The experimenter, using only the flat end of a temperature cylinder cooled to a temperature of 20°C. (Siebrand used 18°-20°), compared two surfaces of equal extent within the square, which included different numbers and different tunings of cold organs. As many comparisons were made as were possible without interfering with other organs. Two series of comparisons gave us the following results. The small letters indicate as before the tunings of the organs compared for intensity.

TABLE III

1st Series:		Obs. C.	2nd Series:		Obs. S.
1 w	<	w m m m i i i	1 w	<	w m m i
2 w m	<	w m m m i i i	2 w	<	w w m m m i
3 w m	<	m m m i	3[w m	<	w m m m]
4 w m i	<	m m m i I	4 w w m	<	m i
5 w m m i	<	m I	5 w w m	<	i
6 w m m i i	<	m m I	6[w i	<	w w m m m]
7 m	<	w m m i	7[i	<	w w m m m]
8[m I	=	w m m i i]	8[i	<	w w m m m]

[5] Siebrand, Untersuchungen über den Kältesinn, *Zsch. f. Sinnesphysiol.*, 1911, XLV, 204-216.

In 11 of the 16 cases a greater intensity is reported for the member which involves an organ of higher sensitivity, the number of components within this member being sometimes more and sometimes less than the other member. In all the 5 remaining cases (in square brackets) the greater intensity appears only when a much larger number of like or slightly lower organs enter into the comparison.

ii. Comparison between a point and a surface. Comparisons were then made in the same way as in the preceding method, except that we now alternatively used the blunted point and the flat end of the temperature cylinders. The results of the two series of experiments are reported in Table IV.

TABLE IV

1st Series:	Obs. C.	2nd Series:	Obs. S.
w w	< m	w m m	> w
w m	< i	w m m	< I
w w m	< i	w i	< I
w w w m	< I	w w m i	> i
w m i i	> i	w m m i	> w
w m i i	> i	w m m i i	> I
m i	> w	w i I	> i
m i	> i	m m	< i
m i	< I	m m i	> i
w m	= i	m i	> I

In 8 of the 20 cases in Table IV we find exceptions to Siebrand's rule that thermal intensity is a function of the number of organs excited, *i.e.*, in these 8 cases the more intensive sensation is produced by the stimulation of a single organ of higher tuning when compared with an area which includes a plurality of spots of lower sensitivity; and in 9 of the 12 remaining cases the "stronger" group contained an organ at least equal in tuning to the single organ.

Although we find but little support for Siebrand's contention, there does here again appear a slight intimation that the number of components does exert an influence upon the judgment of intensity. Let us further refine our method to discover, if it is possible, whether, and under what conditions, the influence of number does obtain.

Method C. In the second part of their fourth method, Barnholt and Bentley have observed the effect of adding an organ of unlike

tuning to one already under stimulation. We have adopted the same procedure, but for another purpose. We wished to discover whether the slight tendency to connect number and intensity was due to the lack of analysis of fused or colligated members. To this end we have distinguished between "total-impression" and "analysis." With the same application of stimulus, we have varied the instruction to our observers. Under (i) below we give our results where the Obs. was instructed to report any intensive change or difference which might appear, without analysis, under stimulation. Under (ii) the instructions were to attend only to the initial cold, reporting any alteration which might occur in that particular sensation.

i. Total-impression judgments. First a "moderate" cold organ (m) was stimulated for 2-3 sec. by a blunt temperature cylinder brought immediately after drying with cotton wool from an ice-bath at 0° C. After an interval of 2-3 sec., the same stimulus was repeated, this time with the addition of a like stimulus applied to an "intensive" (i) or a "weak" (w) organ lying within the radius of 1½ cm. The results in Table V include 110 comparisons for Obs. A, B, and C, and 72 comparisons for a highly trained Obs., M. The figures are percentages. The letters MI and MW give the tuning of the second member in the comparison.

TABLE V

Total impression: with interval: Obs. A, B, C, and M. M with MI or with MW

		G	L	=	?	T'ls
Obs. A, B. and C	MI	58	0	40	2	100
" M	MI	70	2	14	14	100
Obs. A, B, and C	MW	21	3	67	9	100
" M	MW	14	33	42	11	100

The results agree with those in Table II. The addition of "I" leads to a large number of "greater" judgments, and the addition of W to more "less" and "equal" reports. The reduction of "M" under repetition, due to physiological adaptation, doubtless tends to change the whole distribution of cases; but this factor should be constant in both halves of the table.

In order to make still more decided the difference between

total impression and analysis, we repeated the last experiment, except that now we continued the M-sensation by holding the temperature cylinder in place throughout the interval and also as long as the second member (MI or MW) endured. See Table VI, which includes 162 cases for A, B, and C, and 72 for M.

TABLE VI

Total impression: without interval: Obs. A, B, C, and M.

		G	L	=	?	T'ls
Obs. A, B, and C	MI	70	0	26	4	100
" M	MI	75	3	16	6	100
Obs. A, B, and C	MW	8	1	85	6	100
" M	MW	14	19	53	14	100

The results are similar to the last preceding, save that now MI gives more G-cases and MW more equal-cases.

ii. Analytical judgments. The instructions to attend only to the initial cold (M) and to report only an intensive change which might occur in this sensation when I or W was added led to a new distribution in the judgments. Table VII is based upon 90 comparisons for A, B, and C, and 144 for M, under this instruction.

TABLE VII

Analysis: without interval: Obs. A, B, C, and M.

		G	L	=	?	T'ls
Obs. A, B, and C	MI	35	0	62	3	100
" M	MI	15	0	77	8	100
Obs. A, B, and C,	MW	2	0	94	4	100
" M	MW	6	1	92	1	100

It appears that, under analysis, the equal-judgments notably increase. Intensive increases are still reported (though less frequently) when the organ of high tuning (I) is added; but almost never when the organ of low tuning (W) is added. The decreases almost entirely disappear. The tendency is for M to remain unchanged. Obs. M, whose results are most significant on account of his long training in analysis, was encouraged to make as full an introspective report as he could. A closer scrutiny of his equal-cases will throw further light upon the integrity of the cold sensation.[6] This Obs. found that these cases

[6] Cf. Piéron, H., De la discrimination spatiale des sensations thermiques. Son importance pour la théorie générale de la discrimination cutanée, *C. r.*

fell into four classes, to-wit: (1) equal-without-change under addition of I or W (tabulated below as (e), (2) equal-with-an-undefined-addition (e+?), (3) equal-in-the-presence-of-a-stronger cold sensation (e+str.), and (4) equal-in-the-presence-of-a-weaker cold (e+w). The distribution (percentages as before) for Obs. M into these four equal-classes is indicated in Table VIII.

TABLE VIII

Equal-cases (121): Obs. M

	e	e+?	e+str.	e+w	T'ls
MI	27	13	53	7	100
MW	60	21	9	10	100

Thus it appears that although adequate analysis usually (121 in 144 cases) reveals the unchanged initial cold, the addition of a stronger thermal quality is more frequently (53%) noted (*Bemerkt* is Stumpf's term, *loc. cit.* p. 278) than is the addition of a weaker cold (10%), and that a corresponding excess of plain "equals" is shown where the second (added) stimulus excites an organ of *low* sensitivity (60% and 27%). It seems likely (though the matter calls for more explicit treatment) that this result simply exemplifies the fact that the intensive attribute is so closely bound up with attributive clearness that the addition of a stronger member has a more profound effect—other things equal—upon a complex than the addition of a weaker member.[7] The especial significance for our study of the analytic judgments is that the slight tendency to judge as "greater" the temperature (cold) from plural organs is due almost wholly to the tendency to judge in terms of the total impression (as Stumpf found in

Soc. de biol., 1919, lxxxii, 61-65. By dropping water upon the skin, P. excited "pure" thermal sensations (*i.e.*, without pressure). He found the limen for discrimination of locality to be relatively small, 10-15 mm. Most of our *added* stimuli (I or W) fell at approximately this distance from the initial stimulus (M).

[7] The reader will recall that Wundt's doctrine of the "intensive fusions" accords the chief rôle to the most intensive component. The classical instance is the fundamental in the simple clang or musical note. (Wundt, W., *G. d. physiol. Psychol.*, 5th ed., vol. ii, 1902, 418). Cf. the experiments of Bentley upon clearness and intensity in Titchener, E. B., *Lectures on the elementary psychology of feeling and attention*, 1908, 361ff.

the case of tonal complexes, *Tonpsychol.*, ii, 424f). But we cannot solve the problem of thermal increments by reference to total impression alone; and when we analyze, we discover that the thermal quality carries its own intensity in spite of the presence in mind of other sensations of the same mode.

Summary

I. Our experiments support the view that no fixed dependence obtains between thermal intensity and the size of the stimulated area. The apparent and alleged dependence is chiefly explained by reference to the fact of "tuning" of the receptor-organs. Siebrand's contention that thermal sensations are "summated" within the area of stimulation is not substantial because he neglected this cardinal fact.

II. The question of summation demands a more refined method than has yet been used. Barnholt and Bentley have called attention to obscurity and confusion in the statement of the problem and also to various misleading forms of the stimulus error. We have added the distinction between analytic judgments and judgments of total impression, apprehending the necessity of instructing the observer.

III. Aside from the factor of tuning, we have referred the tendency to report as "greater" the cold from two or more organs when compared with one, first, to the judgment of total-impression, as opposed to the judgment based upon explicit analysis, and secondly, to the fact that the most prominent member in a fusion occupies a special prominence, which tends to obscure the other members. Our experiments furnish no evidence of a true summation of sensational elements.